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CONDUCTED BY

WILLIAM STURGEON,

Lecturer on Experimental Philosophy at the Honourable East India
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THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;

AND
Guardian of Experimental Science.

JULY, 1839.

- I. *Experimental Researches in Electricity.—Eleventh Series.* By MICHAEL FARADAY, Esq., D.C.L., F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acad. of Sciences, Paris, Petersburg, Florence, Copenhagen, Berlin, &c., &c.*

Received November 30—Read December 31, 1837.

- §. 18. *On Induction.* ¶ i. *Induction an action of contiguous particles.* ¶ ii. *Absolute charge of matter.* ¶ iii. *Electrometer and inductive apparatus employed.* ¶ iv. *Induction in curved lines.* ¶ v. *Specific inductive capacity.* ¶ vi. *General results as to induction.*

¶ i. *Induction an action of contiguous particles.*

1161. The science of electricity is in that state in which every part of it requires experimental investigation; not merely for the discovery of new effects, but, what is just now of far more importance, the development of the means by which the old effects are produced, and the consequent more accurate determination of the first principles of action of the most extraordinary and universal power in nature:—and to those philosophers who pursue the inquiry zealously yet cautiously, combining experiments with analogy, suspicious of their preconceived notions, paying more respect to a fact than a theory, not too hasty to generalize, and above all things, willing at every step to cross-examine their own opinions, both by reasoning and experiment, no branch of knowledge can afford so fine and ready a field for discovery as this. Such is most abundantly shown to be the case by the progress which electricity has made in the last

* From the Transactions of the Royal Society.

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thirty years: Chemistry and Magnetism have successively acknowledged its overruling influence; and it is probable that every effect depending upon the powers of inorganic matter, and perhaps most of those related to vegetable and animal life, will ultimately be found subordinate to it.

1162. Amongst the actions of different kinds into which electricity has conventionally been subdivided, there is, I think, none which excels or even equals in importance that called *Induction*. It is of the most general influence in electrical phenomena appearing to be concerned in every one of them, and has in reality the character of a first, essential, and fundamental principle. Its comprehension is so important, that I think we cannot proceed much further in the investigation of the laws of electricity without a more thorough understanding of its nature; how otherwise can we hope to comprehend the harmony and even unity of action which doubtless governs electrical excitement by friction, by chemical means, by heat, by magnetic influence, by evaporation, and even by the living being?

1163. In the long-continued course of experimental inquiry in which I have been engaged, this general result has pressed upon me constantly, namely, the necessity of admitting two forces, or two forms or directions of a force (516. 517.), combined with the impossibility of separating these two forces (or electricities) from each other, either in the phenomena of statical electricity or those of the current. In association with this, the impossibility under any circumstances, as yet, of absolutely charging matter of any kind with one or the other electricity dwelt on my mind, and made me wish and search for a clearer view than any that I was acquainted with, of the way in which electrical powers and the particles of matter are related; especially in inductive actions upon which almost all others appeared to rest.

1164. When I discovered the general fact that electrolytes refused to yield their elements to a current when in the solid state though they gave them forth freely if in the liquid condition (380. 394. 402.), I thought I saw an opening to the elucidation of inductive action and the possible subjugation of many dissimilar phenomena to one law. For let the electrolyte be water, a plate of ice being coated with platina foil on its two surfaces, and these coatings connected with any continued source of the two electrical powers, the ice will charge like a Leyden arrangement, presenting a case of common induction, but no current will pass. If the ice be liquefied, the induction will fall to a certain degree, because a current can now pass; but its passing is dependent upon a *peculiar molecular arrangement*

of the particles consistent with transfer of the elements of the electrolyte in opposite directions, the degree of discharge and the quantity of elements evolved being exactly proportioned to each other (377. 783.). Whether the charging of the metallic coating be effected by a powerful electrical machine, a strong and large voltaic battery, or a single pair of plates, makes no difference in the principle, but only in the degree of action (360.). Common induction takes place in each case if the electrolyte be solid, or if fluid chemical action and decomposition ensue, provided opposing actions do not interfere; and it is of high importance occasionally thus to compare effects in their extreme degrees, for the purpose of enabling us to comprehend the nature of an action in its weak state, which may be only sufficiently evident to us in its stronger condition. As, therefore, in the electrolyte, *induction* appeared to be the *first* step, and *decomposition* the *second* (the power of separating these steps from each other by giving the solid or fluid condition being in our hands); as the induction was the same in its nature as that through air, glass, wax, &c. produced by any of the ordinary means; and as the whole effect in the electrolyte appeared to be an action of the particles thrown into a peculiar or polarized state, I was led to suspect that common induction itself was in all cases an *action of contiguous particles*, and that electrical action at a distance (i. e. ordinary inductive action) never occurred except through the intermediate influence of the intervening matter.

1165. The respect which I entertain towards the names of Epinus, Cavendish, Poisson, and other most eminent men, all of whose theories I believe consider induction as an action at a distance and in straight lines, long indisposed me to the view I have just stated; and though I always watched for opportunities to prove the opposite opinion, and made such experiments occasionally as seemed to bear directly on the point, as, for instance, the examination of electrolytes, solid and fluid, whilst under induction by polarized light (951, 955.), it is only of late, and by degrees, that the extreme generality of the subject has urged me still further to extend my experiments and publish my view. At present I believe ordinary induction in all cases to be an action of contiguous particles, consisting in a species of polarity, instead of being an action of either particles or masses at sensible distances: and if this be true, the distinction and establishment of such a truth must be of the greatest consequence to our further progress in the investigation of the nature of electric forces. The linked condition of electrical induction with chemical decomposition; of voltaic excitement with chemical action; the transfer of

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elements in an electrolyte; the original cause of excitement in all cases; the nature and relation of conduction and insulation; of the direct and lateral or transverse action constituting electricity and magnetism; with many other things more or less incomprehensible at present, would all be affected by it, and perhaps receive a full explication in their reduction under one general law.

1166. I searched for an unexceptionable test of my view not merely in the accordance of known facts with it, but in the consequences which would flow from it if true; especially in those which would not be consistent with the theory of action at a distance. Such a consequence seemed to me to present itself in the direction in which inductive action could be exerted. If in straight lines only, though not perhaps decisive, it would be against my view; if in curved lines also, that would be a natural result of the action of contiguous particles, but I think utterly incompatible with action at a distance, as assumed by the received theories, which according to every fact and analogy we are acquainted with, is always in straight lines.

1167. Again, if induction be an action of contiguous particles, and also the first step in the process of electrolyzation (1164, 949,), there seemed reason to expect some particular relation of it to the different kinds of matter through which it would be exerted, or something equivalent to a specific electric induction for different bodies, which, if it existed, would unequivocally prove the dependance of induction on the particles; and though this, in the theory of Poisson and others, has never been supposed to be the case, I was soon led to doubt the received opinion, and have taken great pains in subjecting this matter to close experimental examination.

1168. Another ever-present question on my mind has been whether electricity has an actual and independent existence as a fluid or fluids, or was a mere power of matter, like what we conceive of the attraction of gravitation. If determined either way it would be an enormous advance in our knowledge; and as having the most direct and influential bearing on my notions, I have always sought for experiments which would in any way tend to elucidate that great question. It was in attempts to prove the existence of electricity separate from matter, by giving an independent charge of either positive or negative power to some substance, and the utter failure of all such attempts, whatever substance was used or whatever means of exciting or *evolving* electricity were employed, that first drove me to look upon induction as an action of the particles of matter, each having *both* forces developed in it in exactly equal amount. It is this circumstance, in connexion

with others, which makes me desirous of placing the remarks on absolute charge first, in the order of proof and argument, which I am about to adduce in favour of my view, that electric induction is an action of the contiguous particles of the insulating medium or *di-electric*.

¶ ii. *On the absolute charge of matter.*

1169. Can matter, either conducting or non-conducting, be charged with one electric force independently of the other in the least degree, either in a sensible or latent state?

1170. The beautiful experiments of Coulomb upon the equality of action of *conductors*, whatever their substance, and the residence of *all* the electricity upon their surfaces,* are sufficient, if properly viewed, to prove that *conductors cannot be bodily charged*; and as yet no means of communicating electricity to a conductor so as to relate its particles to one electricity, and not at the same time to the other in exactly equal amount, has been discovered.

1171. With regard to electric or non-conductors, the conclusion does not at first seem so clear. They may easily be electrified bodily, either by communication (1247.) or excitement; but being so charged, every case in succession, when examined, came out to be a case of induction, and not of absolute charge. Thus, glass within conductors could easily have parts not in contact with the conductor brought into an excited state; but it was always found that a portion of the inner surface of the conductor was in an opposite and equivalent state, or that another part of the glass itself was in an equally opposite state, an *inductive* charge and not an *absolute* charge having been acquired.

1172. Well-purified oil of turpentine, which I find to be an excellent liquid insulator for most purposes, was put into a metallic vessel, and being insulated, was charged, sometimes by contact of the metal with the electrical machine, and at others by a wire dipping into the fluid within; but whatever the mode of communication, no electricity of one kind was retained by the arrangement, except what appeared on the exterior surface of the metal, that portion being there only by an inductive action through the air around. When the oil of turpentine was confined in glass vessels, there were at first some appearances as if the fluid did receive an absolute charge of electricity from the charging wire, but these were quickly reduced to cases of common induction jointly through the fluid, the glass, and the surrounding air.

* Mémoires de l'Académie, 1786, pp. 67, 69, 72; 1787. p. 452.

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1173. I carried these experiments on with air to a very great extent. I had a chamber built, being a cube of twelve feet in the side. A slight cubical wooden frame was constructed, and copper wire passed along and across it in various directions, so as to make the sides a large net-work, and then all was covered in with paper, placed in close connexion with the wires, and supplied in every direction with bands of tin-foil, that the whole might be brought into good metallic communication, and rendered a free conductor in every part. This chamber was insulated in the lecture-room of the Royal Institution; a glass tube about six feet in length was passed through its side, leaving about four feet within and two feet on the outside, and through this a wire passed from the large electrical machine (290) to the air within. By working the machine, the air within this chamber could be brought into what is considered a highly electrified state (being, in fact, the same state as that of the air of a room in which a powerful machine is in operation) and at the same time the outside of the insulated cube was everywhere strongly charged. But putting the chamber in communication with the perfect discharging train described in a former series (292.), and working the machine so as to bring the air within to its utmost degree of charge, if I quickly cut off the connexion with the machine, and at the same moment or instantly after insulated the cube, the air within had not the least power to communicate a further charge to it. If any portion of the air was electrified, as glass or other insulators may be charged (1171), it was accompanied by a corresponding opposite action *within* the cube, the whole effect being merely a case of induction. Every attempt to charge air bodily and independently with the least portion of either electricity failed.

1174. I put a delicate gold-leaf electrometer within the cube, and then charged the whole by an *outside* communication, very strongly, for some time together; but neither during the charge or after the discharge did the electrometer or air within show the least sign of electricity. I charged and discharged the whole arrangement in various ways, but in no case could I obtain the least indication of an absolute charge; or of one by induction in which the electricity of one kind had the smallest superiority in quantity over the other. I went into the cube and lived in it, and using lighted candles, electrometers, and all other tests of electrical states, I could not find the least influence upon them, or indication of anything particular given by them, though all the time the outside of the cube was powerfully charged, and large sparks and brushes were darting off from every part of its outer surface. The conclu-

sion I have come to is, that non-conductors, as well as conductors, have never yet had an absolute and independent charge of one electricity communicated to them, and that to all appearance such a state of matter is impossible.

1175. There is another view of this question which may be taken under the supposition of the existence of an electric fluid or fluids. It may be impossible to have the one fluid or state in a free condition without its producing by induction the other, and yet possible to have cases in which an insulated portion of matter in one condition being uncharged, shall, by a change of state, evolve one electricity or the other: and though such evolved electricity might immediately induce the opposite state in its neighbourhood, yet the mere evolution of one electricity without the other in the *first instance*, would be a very important fact in the theory which assumes a fluid or fluids; these theories as I understand them assigning not the slightest reason why such an effect should not occur.

1176. But on searching for such cases I cannot find one. Evolution by friction, as is well known, gives both powers in equal proportion. So does evolution by chemical action, notwithstanding the great diversity of bodies which may be employed, and the enormous quantity of electricity which can in this manner be evolved (371. 376. 861. 868.). The more promising cases of change of state, whether by evaporation, fusion, or the reverse processes, still give both forms of the power in *equal* proportion; and the cases of splitting of mica and other crystals, the breaking of sulphur, &c. &c., are subject to the same limitation.

1177. As far as experiment has proceeded, it appears, therefore, impossible either to evolve or make disappear one electric force without equal and corresponding change in the other. It is also equally impossible experimentally to charge a portion of matter with one electric force independently of the other. Charge always implies *induction*, for it can in no instance be effected without; and also the presence of the *two* forms of power, equally at the moment of development and afterwards. There is no *absolute* charge of matter with one fluid; no latency of a single electricity. This though a negatively result is an exceedingly important one, being probably the consequence of a natural impossibility, which will become clear to us when we understand the true condition and theory of the electric power.

1178. The preceding considerations already point to the following conclusions: bodies cannot be charged absolutely, but only negatively, and by a principle which is the same with that of *induction*. All *charge* is sustained by induction. All

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phenomena of *intensity* include the principle of induction. All *excitation* is dependent on or directly related to induction. All *currents* involve previous intensity and therefore previous induction. INDUCTION appears to be the essential function both in the first development and the consequent phenomena of electricity.

¶ iii. *Electrometer and inductive apparatus employed.*

1179. Leaving for a time the further consideration of the preceding facts until they can be collated with other results bearing directly on the great question of the nature of induction, I will now describe the apparatus I have had occasion to use; and in proportion to the importance of the principles sought to be established is the necessity of doing this so clearly as to leave no doubt of the results behind.

1180. *Electrometer.* The measuring instrument I have employed has been the torsion balance electrometer of Coulomb, constructed, generally, according to his instructions,* but with certain variations and additions, which I will briefly describe. The lower part was a glass cylinder eight inches in height and eight inches in diameter; the tube for the torsion thread was seventeen inches in length. The torsion thread itself was not of metal, but glass, according to the excellent suggestion of the late Dr. Ritchie.† It was twenty inches in length, and of such tenuity that when the shell lac lever and attached ball, &c. were connected with it, they made about ten vibrations in a minute. It would bear torsion through four revolutions, or 1440° , and yet when released, return accurately to its position; probably it would have borne considerably more than this without injury. The repelled ball was of pith, gilt, and was 0.3 of an inch in diameter. The horizontal stem or lever supporting it was of shell lac, according to Coulomb's direction, the arm carrying the ball being 2.4 inches long and the other only 1.2 inches: to this was attached the vane, also described by Coulomb, which I found to answer admirably its purpose of quickly destroying vibrations. That the inductive action within the electrometer might be uniform in all positions of the repelled ball and in all states of the apparatus, two bands of tin foil, about an inch wide each, were attached to the inner surface of the glass cylinder, going entirely round it at a distance of 0.4 of an inch from each other, and at such a height that the intermediate clear surface was in the same horizontal plane with the lever and ball. These bands

* Mémoires de l'Académie, 1785, p. 570.

† Phil. Trans., 1830.

were connected with each other and with the earth, and, being perfect conductors, always exerted a uniform influence on the electrified balls within, which the glass surface, from its irregularity of condition at different times, I found, did not. For the purpose of keeping the air within the electrometer in a constant state as to dryness, a glass dish, of such size as to enter easily within the cylinder, had a layer of fused potash placed within it, and this being covered with a disc of fine wire gauze to render its inductive action uniform at all parts, was placed within the instrument at the bottom and left there.

1181. The moveable ball used to take and measure the portion of electricity under examination, and which may be called the *repelling*, or the *carrier*, ball, was of soft alder wood, well and smoothly gilt. It was attached to a fine shell lac stem, and introduced through a hole into the electrometer according to Coulomb's method: the stem was fixed at its upper end in a block or vice, supported on three short feet: and on the surface of the glass cover above was a plate of lead with stops on it, so that when the carrier ball was adjusted in its right position, with the vice above bearing at the same time against these stops, it was perfectly easy to bring away the carrier ball and restore it to its place again very accurately, without any loss of time.

1182. It is quite necessary to attend to certain precautions respecting these balls. If of pith alone they are bad; for when very dry, that substance is so imperfect a conductor that it neither receives nor gives a charge freely, and so, after contact with a charged conductor, is liable to be in an uncertain condition. Again, it is difficult to turn pith so smoothly as to leave the ball, even when gilt, sufficiently free from irregularities of form, as to retain its charge undiminished for a considerable length of time. When therefore the balls are finally prepared and gilt they should be examined, and being electrified, unless they can hold their charge with very little diminution for a considerable time, and yet be discharged instantly and perfectly by the touch of an uninsulated conductor, they should be dismissed.

1183. It is, perhaps, unnecessary to refer to the graduation of the instrument, further than to explain how the observations were made. On a circle or ring of paper on the outside of the glass cylinder, fixed so as to cover the internal lower ring of tin foil, were marked four points corresponding to angles of 90° ; four other points exactly corresponding to these points being marked on the upper ring of tin foil within. By these and the adjusted screws, on which the whole instrument stands, the glass torsion thread could be brought accurately

into the centre of the instrument and of the graduations on it. From one of the four points on the exterior of the cylinder a graduation of 90° was set off, and a corresponding graduation was placed upon the upper tin foil on the opposite side of the cylinder within; and a dot being marked on that point of the surface of the repelled ball nearest to the side of the electrometer, it was easy, by observing the line which this dot made with the lines of the two graduations just referred to, to ascertain accurately the position of the ball. The upper end of the glass thread was attached, as in Coulomb's original electrometer, to an index, which had its appropriate graduated circle, upon which the degree of torsion was ultimately to be read off.

1184. After the levelling of the instrument and adjustment of the glass thread, the blocks which determine the place of the *carrier ball* are to be regulated (1181) so that, when the carrier arrangement is placed against them, the centre of the ball may be in the radius of the instrument corresponding to 0° on the lower graduation or that on the side of the electrometer, and at the same level and distance from the centre as the *repelled ball* on the suspended torsion lever. Then the torsion index is to be turned until the ball connected with it (the repelled ball) is accurately at 30° , and finally the graduated arch belonging to the torsion index is to be adjusted so as to bring 0° upon it to the index. This state of the instrument was adopted as that which gave the most direct expression of the experimental results, and in the form having fewest variable errors; the angular distance of 30° being always retained as the standard distance to which the balls were in every case to be brought, and the whole of the torsion being read off at once on the graduated circle above. Under these circumstances the distance of the balls from each other was not merely the same in degree, but their position in the instrument, and in relation to every part of it, was actually the same every time that a measurement was made; so that all irregularities arising from slight difference of form and action in the instrument and the bodies around were avoided. The only difference which could occur in the position of anything within, consisted in the deflexion of the torsion thread from a vertical position, more or less, according to the force of repulsion of the balls; but this was so slight as to cause no interfering difference in the symmetry of form within the instrument, and gave no error in the amount of torsion force indicated on the graduation above.

1185. Although the constant angular distance of 30° between the centres of the balls was adopted, and found abun-

dantly sensible, for all ordinary purposes, yet the facility of rendering the instrument far more sensible, by diminishing this distance was at perfect command ; the results at different distances being very easily compared with each other either by experiment, or, as they are inversely as the squares of the distances, by calculation.

1186. The Coulomb balance electrometer requires experience to be understood ; but I think it a very valuable instrument in the hands of those who will take pains by practice and attention to learn the precautions needful in its use. Its insulating condition varies with circumstances, and should be examined before it is employed in experiments. In an ordinary and fair condition, when the balls were so electrified as to give a repulsive torsion force of 400° at the standard distance of 30° it took nearly four hours to sink to 50° at the same distance ; the average loss from 400° to 300° being at the rate of $2^\circ\cdot7$ per minute, from 300° to 200° of $1^\circ\cdot7$ per minute, from 200° to 100° of $1^\circ\cdot3$ per minute, and from 100° to 50° of $0^\circ\cdot87$ per minute. As a complete measurement by the instrument may be made in much less than a minute, the amount of loss in that time is but small, and can easily be taken into account.

1187. *The inductive apparatus.*—My object was to examine inductive action carefully when taking place through different media, for which purpose it was necessary to subject these media to it in exactly similar circumstances, and in such quantities as should suffice to eliminate any variations they might present. The requisites of the apparatus to be constructed were, therefore, that the inducing surfaces of the conductors should have a constant form and state, and be at a constant distance from each other ; and that either solids, or fluids, or gases might be placed and retained between these surfaces with readiness and certainty, and for any length of time.

1188. The apparatus used may be described in general terms as consisting of two metallic spheres of unequal diameter, placed, the smaller within the larger, and concentric with it ; the interval between the two being the space through which the induction was to take place. A section of it is given (fig. 1, Plate I.) *a, a*, are the two halves of a brass sphere, with an air-tight joint at *b*, like that of the Magdeburg hemispheres, made perfectly flush and smooth inside so as to present no irregularity ; *c* is a connecting piece by which the apparatus is joined to a good stop-cock *d*, which is itself attached either to the metallic foot *e*, or to an air pump. The aperture within the hemisphere at *f* is very small ; *g* is a brass collar fitted to the upper hemisphere, through which the shell lac support of the inner ball and its

stem passes; h is the inner ball, also of brass; it screws on to a brass stem i , terminated above by a brass ball B ; l, l is a mass of shell lac, moulded carefully on to i , and serving both to support and insulate it and its balls h, B . The shell-lac stem l is fitted into the socket g , by a little ordinary resinous cement, more fusible than shell lac, applied at $m m$ in such a way as to give sufficient strength and render the apparatus air-tight there, yet leave as much as possible of the lower part of the shell-lac stem untouched, as an insulation between the ball h and the surrounding sphere a, a . The ball h has a small aperture at n , so that when the apparatus is exhausted of one gas and filled with another, the ball h may itself also be exhausted and filled, that no variation of the gas in the interval o may occur during the course of an experiment.

1189. The inner ball has a diameter of 2.33 inches, and the surrounding sphere an internal diameter of 3.57 inches. Hence the width of the intervening space, through which the induction is to take place, is 0.62 of an inch; and the extent of this place or plate, i.e. the surface of a medium sphere, may be taken as twenty-seven square inches, a quantity considered as sufficiently large for the comparison of different substances. Great care was taken in finishing well the inducing surfaces of the ball h and sphere a, a ; and no varnish or lacquer was applied to them, or to any part of the metal of the apparatus.

1190. The attachment and adjustment of the shell-lac stem was a matter requiring considerable care, especially as, in consequence of its cracking, it had frequently to be renewed. The best lac was chosen and applied to the wire i , so as to be in good contact with it everywhere, and in perfect continuity throughout its own mass. It was not thinner than is given by proportion in the drawing, for when less it frequently cracked within a few hours after its cooling. I think that very slow cooling or annealing improved its quality in this respect. The collar g was made as thin as could be, that the lac might be as large there as possible. In order that at every re-attachment of the stem to the upper hemisphere the ball h might have the same relative position, a gauge p (fig. 2) was made of wood, and this being applied to the ball and hemisphere whilst the cement at m was still soft, the bearings of the ball at $q q$, and the hemisphere at $r r$, were forced home, and the whole left until cold. Thus all difficulty in the adjustment of the ball in the sphere was avoided.

1191. I had occasion at first to attach the stem to the socket by other means, as a band of paper or a plugging of white silk thread; but these were very inferior to the cement, interfering much with the insulating power of the apparatus.

1192. The retentive power of this apparatus was, when in good condition, better than that of the electrometer (1186), i. e. the proportion of loss of power was less. Thus when the apparatus was electrified, and also the balls in the electrometer, to such a degree, that after the inner ball had been in contact with the top of *k* of the ball of the apparatus, it caused a repulsion indicated by 600° of torsion force, then in falling from 600° to 400° the average loss was 8.6 per minute; from 400° to 300° the average loss was 2.6 per minute; from 300° to 200° it was 1.7 per minute; from 200° to 170° it was 1° per minute. This was after the apparatus had been charged for a short time; at the first instant of charging there is an apparent loss of electricity, which can only be comprehended hereafter (1207. 1250.).

1193. When the apparatus loses its insulating power suddenly, it is almost always from a crack near to or within the brass socket. These cracks are usually transverse to the stem. If they occur at the part attached by common cement to the socket, the air cannot enter, and being then as a vacua, they conduct away the electricity and lower the charge, as fast almost as if a piece of metal had been introduced there. Occasionally stems in this state, being taken out and cleared from the common cement, may, by the careful application of the heat of a spirit lamp, be so far softened and melted as to renew perfect continuity of the parts; but if that does not succeed in restoring things to a good condition, the remedy is a new shell-lac stem.

1194. The apparatus when in order could easily be exhausted of air and filled with any given gas; but when that gas was acid or alkaline, it could not properly be removed by the air-pump, and yet required to be perfectly cleared away. In such cases the apparatus was opened and cleared; and with respect to the inner ball *h*, it was washed out two or three times with distilled water introduced at the screw hole, and then being heated above 212° , air was blown through to render the interior perfectly dry.

1195. The inductive apparatus described is evidently a Leyden phial, with the advantage, however, of having the dielectric or insulating medium changed at pleasure. The balls *k* and *B*, with the connecting wire *i*, constitute the charged conductor, upon the surface of which all the electric force is resident by virtue of induction (1178). Now though the largest portion of this induction is between the ball *k* and the surrounding sphere *a*, yet the wire *i* and the ball *B* determine a part of the induction from their surfaces towards the external surrounding conductors. Still, as all things in that

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respect remain the same, whilst the medium within at *o o* may be varied, any changes exhibited by the whole apparatus will in such cases depend upon the variations made in the interior; and it was these changes I was in search of, the negation or establishment of such differences being the great object of my inquiry. I considered that these differences, if they existed, would be most distinctly set forth by having two apparatus of the kind described, precisely similar in every respect; and then, different insulating media being within, to charge one and measure it, and after dividing the charge with the other, to observe what the ultimate conditions of both were. If insulating media really had any specific differences in favouring or opposing inductive action through them, such differences, I conceived, could not fail of being developed by such a process.

1196. I will wind up this description of the apparatus, and explain the precautions necessary in their use, by describing the form and order of the experiments made to prove their equality when both contained common air. In order to facilitate reference I will distinguish the two by the terms App. i. and App. ii.

1197. The electrometer is first to be adjusted and examined (1184), and the app. i. and ii. are to be perfectly discharged. A Leyden phial is to be charged to such a degree that it would give a spark of about one sixteenth or one twentieth of an inch in length between two balls of half an inch diameter; and the carrier ball of the electrometer being charged by this phial, is to be introduced into the electrometer, and the lever ball brought by the motion of the torsion index against it; the charge is thus divided between the balls, and repulsion ensues. It is useful then to bring the repelled ball to the standard distance of 30° by the motion of the torsion index, and observe the force in degrees required for this purpose; this force will in future experiments be called *repulsion of the balls*.

1198. One of the inductive apparatus, as for instance, app. i., is now to be charged from the Leyden phial, the latter being in the state it was in when used to charge the balls; the carrier ball is to be brought into contact with the top of its upper ball (*k. fig. 1*), then introduced into the electrometer, and the repulsive force (at the distance of 30°) measured. Again, the carrier should be applied to the app. i. and the measurement repeated; the apparatus i. and ii. are then to be joined, so as to *divide* the charge, and afterwards the force of each measured by the carrier ball, applied as before, and the results carefully noted. After this both i. and ii. are to be discharged; then app. ii. charged, measured, divided with

app. i., and the force of each again measured and noted. If in each case the half charges of app. i. and ii. are equal, and are together equal to the whole charge before division, then it may be considered as proved that the two apparatus are precisely equal in power, and fit to be used in cases of comparison between different insulating media or *dielectrics*.

1199. But the *precautions* necessary to obtain accurate results are numerous. The apparatus i. and ii. must always be placed on a thoroughly uninsulating medium. A mahogany table, for instance, is far from satisfactory in this respect, and therefore a sheet of tin foil, connected with an extensive discharging train (292.), is what I have used. They must be so placed also as not to be too near each other, and yet equally exposed to the inductive influence of surrounding objects; and these objects, again, should not be disturbed in their position during an experiment, or else variations of induction upon the external ball B of the apparatus may occur, and so errors be introduced into the results. The carrier ball, when receiving its portion of electricity from the apparatus, should always be applied at the same part of the ball, as, for instance, the summit *k*, and always in the same way; variable induction from the vicinity of the head, hands, &c. being avoided, and the ball after contact being withdrawn upwards in a regular and constant manner.

1200. As the stem had occasionally to be changed (1190.), and the change might occasion slight variations in the position of the ball within, I made such a variation purposely, to the amount of an eighth of an inch (which is far more than ever could occur in practice), but did not find that it sensibly altered the relation of the apparatus, or its inductive condition *as a whole*. Another trial of the apparatus was made as to the effect of dampness in the air, one being filled with very dry air, and the other with air from over water. Though this produced no change in the result, except an occasional tendency to more rapid dissipation, yet the precaution was always taken when working with gases (1290.) to dry them perfectly.

1201. It is essential that the interior of the apparatus should be *perfectly* free from dust or small loose particles, for these very rapidly lower the charge and interfere on occasions when their presence and action would hardly be expected. To breathe on the interior of the apparatus and wipe it out quietly with a clean silk handkerchief, is an effectual way of removing them; but then the intrusion of other particles should be carefully guarded against, and a dusty atmosphere should for this and several other reasons be avoided.

1202. The shell lac stem requires occasionally to be well wiped, to remove, in the first instance, the film of wax and adhering matter which is upon it; and afterwards to displace dirt and dust which will gradually attach to it in the course of experiments. I have found much to depend upon this precaution, and a silk handkerchief is the best wiper.

1203. But wiping and some other circumstances tend to give a charge to the surface of the shell lac stem. This should be removed, for, if allowed to remain, it very seriously affects the degree of charge given to the carrier ball by the apparatus (1232). This condition of the stem is best observed by discharging the apparatus, applying the carrier ball to the stem, touching it with the finger, insulating and removing it, and examining whether it has received any charge (by induction) from the stem; if it has, the stem itself is in a charged state. The best method of removing the charge I have found to be, to cover the finger with a single fold of a silk handkerchief, and breathing on the stem, to wipe it immediately after with the finger, the ball B and its connected wire, &c. being at the same time *uninsulated*: the wiping place of the silk must not be changed; it then becomes sufficiently damp not to excite the stem, and is yet dry enough to leave it in a clean and excellent insulating condition. If the air be dusty, it will be found that a single charge of the apparatus will bring on an electric state of the outside of the stem, in consequence of the carrying power of the particles of dust; whereas in the morning, and in a room which has been left quiet, several experiments can be made in succession without the stem assuming the least degree of charge.

1204. Experiments should not be made by candle or lamp light except with much care, for flames have great and yet unsteady powers of affecting and dissipating electrical charges.

1205. As a final observation on the state of the apparatus, they should retain their charge well and uniformly, and alike for both, and at the same time allow of a perfect and instantaneous discharge, giving them no charge to the carrier ball, whatever part of the ball B it may be applied to (1218.).

1206. With respect to the balance electrometer all the precautions that need be mentioned, are, that the carrier ball is to be preserved during the first part of an experiment in its electrified state, the loss of electricity which would follow upon its discharge being avoided; and, that in introducing it into the electrometer through the hole in the glass plate above, care should be taken that it do not touch, or even come near to, the edge of the glass.

1207. When the whole charge in one apparatus is divided between the two, the gradual fall, apparently from dissipation, in the apparatus which has *received* the half charge is greater than in the one *originally* charged. This is due to a peculiar effect to be described hereafter (1250. 1251.), the interfering influence of which may be avoided to a great extent by going through the steps of the process regularly and quickly; therefore, after the original charge has been measured, in app. i. for instance, i. and ii. are to be symmetrically joined by their balls B, the carrier touching one of these balls at the same time; it is first to be removed, and then the apparatus separated from each other; app. ii. is next quickly to be measured by the carrier, then app. i.; lastly, ii. is to be discharged, and the discharged carrier applied to it to ascertain whether any residual effect is present (1205.), and app. i. being discharged is also to be examined in the same manner and for the same purpose.

1208. The following is an example of the division of a charge by the two apparatus, air being the dielectric in both of them. The observations are set down one under the other in the order in which they were taken, the left hand numbers representing the observations made on app. i. and the right hand numbers those on app. ii. App. i. is that which was originally charged, and after two measurements, the charge was divided with app. ii.

App. i.	App. ii.
Balls 160°	
254°	0°
250	
divided and instantly taken	
124	122
1	after being discharged.
	2 after being discharged.

1209. Without endeavouring to allow for the loss which must have been gradually going on during the time of the experiment, let us observe the results of the numbers as they stand. As 1° remained in app. i. in an undischargable state, 249° may be taken as the utmost amount of the transferable or divisible charge, the half of which is 124°·5. As app. ii. was free of charge in the first instance, and immediately after the division was found with 122°, this amount *at least* may be taken as what it had received. On the other hand 124° minus 1°, or 123°, may be taken as the half of the transferable charge

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retained by app. i. Now these do not differ much from each other, or from $124^{\circ} \cdot 5$, the half of the full amount of transferable charge; and when the gradual loss of charge evident in the difference between 254° and 250° of app. i. is also taken into account, there is every reason to admit the result as showing an equal division of charge, *unattended by any disappearance of power* except that due to dissipation.

1210. I will give another result, in which app. ii. was first charged, and where the residual action of that apparatus was greater than in the former case.

App. i.	Balls	App. ii.
	150°	
.		152°
.		148
divided and instantly taken		
70°		78
.		5 immediately after discharge.
0		immediately after discharge.

1211. The transferable charge being $148^{\circ} - 5^{\circ}$, its half is $71^{\circ} \cdot 5$, which is not far removed from 70° , the half charge of i.; or from 73° , the half charge of ii.: these half charges again making up the sum of 143° , or just the amount of the whole transferable charge. Considering the errors of experiment, therefore, these results may again be received as showing that the apparatus were equal in inductive capacity, or in their powers of receiving charges.

1212. The experiments were repeated with charges of negative electricity, with the same general results.

1213. That I might be sure of the sensibility and action of the apparatus, I made such a change in one as ought upon principle to increase its inductive force, i. e. I put a metallic lining into the lower hemisphere of app. i., so as to diminish the thickness of the intervening air in that part, from $0 \cdot 62$ to $0 \cdot 435$ of an inch: this lining was carefully shaped and rounded so that it should not present a sudden projection within at its edge, but a gradual transition from the reduced interval in the lower part of the sphere to the larger one in the upper.

1214. This change immediately caused app. i. to produce effects indicating that it had a greater aptness or capacity for induction than app. ii. Thus, when a transferable charge in app. ii. of 469° was divided with app. i., the former retained a charge of 225° , whilst the latter showed one of 227° , i. e. the former had lost 244° in communicating 227° to the latter: on the other hand, when app. i. had a transferable charge in

it of 381° divided by contact with app. ii., it lost 181° only, whilst it gave to app. ii. as many as 194° :—the sum of the divided forces being in the first instance *less*, and in the second instance *greater* than the original undivided charge. These results are the more striking, as only one half of the interior of app. i. was modified, and they show that the instruments are capable of bringing out differences in inductive force from amongst the errors of experiment, when these differences are much less than that produced by the alteration made in the present instance.

¶ iv. *Induction in curved lines.*

1215. Amongst those results deduced from the molecular view of induction (1166.), which, being of a peculiar nature, are the best tests of the truth or error of the theory, the expected action in curved lines is, I think, the most important at present; for, if shown to take place in an unexceptionable manner, I do not see how the old theory of action at a distance and in straight lines can stand, or how the conclusion that ordinary induction is an action of contiguous particles can be resisted.

1216. There are many forms of old experiments which might be quoted as favourable to, and consistent with the view I have adopted. Such are most cases of electro-chemical decomposition, electrical brushes, auras, sparks, &c.; but as these might be considered equivocal evidence, inasmuch as they include a current and discharge (though they have long been to me indications of prior molecular action (1230.)), I endeavoured to devise such experiments for first proofs as should not include transfer, but relate altogether to the pure simple inductive action of statical electricity.

1217. It was also of importance to make these experiments in the simplest possible manner, using not more than one insulating medium or dielectric at a time, lest differences of slow conduction should produce effects which might erroneously be supposed to result from induction in curved lines. It will be unnecessary to describe the steps of the investigation minutely; I will at once proceed to the simplest mode of proving the facts, first in air and then in other insulating media.

1218. A cylinder of solid shell-lac, 0.9 of an inch in diameter and seven inches in length, was fixed upright in a wooden foot (fig. 3.): it was made concave or cupped at its upper extremity so that a brass ball or other small arrangement could stand upon it. The upper half of the stem having been excited *negatively* by friction with warm flannel, a brass

ball, B, 1 inch in diameter, was placed on the top, and then the whole arrangement examined by the carrier ball and Coulomb's electrometer (1180. &c.). For this purpose the balls of the electrometer were charged *positively* to about 360° , and then the carrier being applied to various parts of the ball B, the two were uninsulated whilst in contact or in position, then insulated,* separated, and the charge of the carrier examined as to its nature and force. Its electricity was always positive, and its force at the different positions *a*, *b*, *c*, *d*, &c. (fig. 3. and 4.) observed in succession, was as follows :

at <i>a</i>	above 1000°
<i>b</i> it was	149
<i>c</i>	270
<i>d</i>	512
<i>b</i>	130

1219. To comprehend the full force of these results, it must first be understood, that all the charges of the ball B and the carrier are charges by induction, from the action of the excited surface of the shell lac cylinder ; for whatever electricity the ball B received by *communication* from the shell lac, either in the first instance or afterwards, was removed by the uninsulating contacts, only that due to induction remaining ; and this is shown by the charges taken from the ball in this its uninsulated state being always positive, or of the contrary character to the electricity of the shell-lac. In the next place the charges at *a*, *c*, and *d* were of such a nature as might be expected from an inductive action in straight lines, but that obtained at *b* is *not so* : it is clearly a charge by induction, but *induction in a curved line* ; for the carrier ball whilst applied to *b*, and after its removal to a distance of six inches or more from B, could not, in consequence of the size of B, be connected by a straight line with any part of the excited and inducing shell-lac.

1220. To suppose that the upper part of the *uninsulated* ball B, should in some way be retained in an electrified state by that portion of the surface which is in sight of the shell-lac, would be in opposition to what we know already of the subject. Electricity is retained upon the surface of conductors only by induction (1178.) ; and though some persons may not be pre-

* It can hardly be necessary for me to say here, that whatever general state the carrier ball acquired in any place where it was uninsulated and then insulated, it retained on removal from that place, notwithstanding that it might pass through other places, that would have given to it, if uninsulated, a different condition.

pared as yet to admit this with respect to insulated conductors all will as regards uninsulated conductors like the ball B, and to decide the matter we have only to place the carrier ball at *e* (fig. 4.), so that it shall not come in contact with B, uninsulate it by a metallic rod descending perpendicularly, insulate it, remove it, and examine its state: it will be found charged with the same kind of electricity as, and even to a higher degree (1224.) than, if it had been in contact with the summit of B.

1221. To suppose, again, that induction acts in some way *through or across* the metal of the ball, is negatived by the simplest considerations; but a fact in proof will be better. If instead of the ball B a small disc of metal be used, the carrier may be charged at, or above the middle of its upper surface; but if the plate be enlarged to about $1\frac{1}{2}$ or 2 inches in diameter, C (fig. 5.), then no charge will be given to the carrier at *f*, though when applied nearer to the edge at *g*, or even *above the middle* at *h*, a charge will be obtained; and this is true though the plate may be a mere thin film of gold-leaf. Hence it is clear that the induction is not *through* the metal, but through the air or dielectric, and that in curved lines.

1222. I had another arrangement, in which a wire passing downwards through the middle of the shell-lac cylinder to the earth, was connected with the ball B (fig. 6.) so as to keep it in a constantly uninsulated state. This was a very convenient form of apparatus, and the results with it were the same as those described.

1223. In another case the ball B was supported by a shell-lac stem, independently of the excited cylinder of shell-lac, and at half an inch distance from it; but the effects were the same. Then the brass ball of a charged Leyden jar was used in place of the excited shell-lac to produce induction; but this caused no alteration of the phenomena. Both positive and negative inducing charges were tried with the same general results. Finally, the arrangement was inverted in the air for the purpose of removing every possible objection to the conclusions, but they came out exactly the same.

1224. Some results obtained with a brass hemisphere instead of the ball B were exceedingly interesting. It was $1\frac{3}{16}$ of an inch in diameter, (fig. 7.), and being placed on the top of the excited shell-lac cylinder, the carrier ball was applied, as in the former experiments (1218.), at the respective positions delineated in the figure. At *i* the force was 112° , at *k* 108° , at *l* 65° , at *m* 35° ; the inductive force gradually diminishing, as might have been expected, to this point.

But on raising the carrier to the position *n* the charge increased to 87° ; and on raising it still higher to *o*, the charge still further increased to 105° : at a higher point still, *p*, the charge taken was smaller in amount, being 98° , and continued to diminish for more elevated positions. Here the induction fairly turned a corner. Nothing, in fact, can better show both the curved lines or courses of the inductive action, disturbed as they are from their rectilineal form by the shape, position, and condition of the metallic hemisphere; and also a *lateral tension*, so to speak, of these lines on one another: all depending, as I conceive, on induction being an action of the contiguous particles of the dielectric thrown into a state of polarity and tension, and mutually related by their forces in all directions.

1225. As another proof that the whole of these actions were inductive, I may state a result which was exactly what might be expected, namely, that if uninsulating conducting matter was brought round and near to the excited shell-lac stem, then the inductive force was directed towards it, and could not be found on the top of the hemisphere. Removing this matter the lines of force resumed their former direction. The experiment affords proofs of the lateral tension of these lines, and supplies a warning to remove such matter in repeating the above investigation.

1226. After these results on curved inductive action in air I extended the experiments to other gases, using first carbonic acid and then hydrogen: the phenomena were precisely those already described. In these experiments I found that if the gases were confined in vessels they required to be very large, for whether of glass or earthenware, the conducting power of such materials is so great that the induction of the excited shell-lac cylinder towards them is as much as if they were metal; and if the vessels be small, so great a portion of the inductive force is determined towards them that the lateral tension or mutual repulsion of the lines of force before spoken of (1224.), by which their inflection is caused, is so much relieved in other directions, that no inductive charge will be given to the carrier ball in the positions *k, l, m, n, o, p*, (fig. 7.). A very good mode of making the experiment is to let large currents of the gases ascend or descend through the air, and carry on the experiments in these currents.

1227. These experiments were then varied by the substitution of a liquid dielectric, namely, *oil of turpentine*, in place of air and gases. A dish of thin glass well covered with a film of shell-lac (1272.), and found by trial to insulate well, had some highly rectified oil of turpentine put into it to the

depth of half an inch, and being then placed upon the top of the brass hemisphere, (fig. 7.) observations were made with the carrier ball as before (124.). The results were the same, and the circumstance of some of the positions being within the fluid and some without, made no sensible difference.

1228. Lastly, I used a few solid dielectrics for the same purpose, and with the same results. These were shell-lac, sulphur, fused and cast borate of lead, flint glass well covered with a film of lac, and spermaceti. The following was the form of experiment with sulphur, and all were of the same kind. A square plate of the substance, two inches in extent and 0.6 of an inch in thickness, was cast with a small hole or depression in the middle of one surface to receive the carrier ball. This was placed upon the surface of the metal hemisphere (fig. 9.) arranged on the excited lac as in former cases, and observations were made at *n*, *o*, *p*, and *q*. Great care was required in these experiments to free the sulphur or other solid substance from any charge it might previously have received. This was done by breathing and wiping (1203.), and the substance being found free from all electrical excitement, was then used in the experiment; after which it was removed and again examined, to ascertain that it had received no charge, but had acted really as a dielectric. With all these precautions the results were the same; and it is thus very satisfactory to obtain the curved inductive action through *solid bodies*, as any possible effect from the translation of charged particles in fluids or gases, which some persons might imagine to be the case, is here entirely negated.

1229. In these experiments with solid dielectrics, the degree of charge, assumed by the carrier ball at the situations *n*, *o*, *p* (fig. 9.), was decidedly greater than that given to the ball at the same places when air only intervened between it and the metal hemisphere. This effect is consistent with what will hereafter be found to be the respective relations of these bodies, as to their power of facilitating induction through them (1269. 1273. 1277.).

1230. I might quote *many* other forms of experiment, some old and some new, in which induction in curved or contorted lines takes place, but think it unnecessary after the preceding results; I shall therefore mention but two. If a conductor A, (fig. 8.) be electrified, and an uninsulated metallic ball B, or even a plate, provided the edges be not too thin, be held before it, a small electrometer at *c* or at *d*, uninsulated, will give signs of electricity, opposite in its nature to that of A, and therefore caused by induction, although the influencing and influenced bodies cannot be joined by a right line passing

through the air. Or if, the electrometers being removed, a point be fixed at the back of the ball in its uninsulated state as at C, this point will become luminous and discharge the conductor A. The latter experiment is described by Nicholson,* who, however, reasons erroneously upon it. As to its introduction here, though it is a case of discharge, the discharge is preceded by induction, and that induction must be in curved lines.

1231. As argument against the received theory of induction and in favour of that which I have ventured to put forth, I cannot see how the preceding results can be avoided. The effects are clearly inductive effects produced by electricity, not in currents but in its statical state, and this induction is exerted in lines of force which, though in many experiments they may be straight, are here curved more or less according to circumstances. I use the term *line of inductive force* merely as a temporary conventional mode of expressing the direction of the power in cases of induction; and in the experiments with the hemisphere (1224.), it is curious to see how, when certain lines have terminated on the under surface and edge of the metal, those which were before lateral to them *expand and open out from each other*, some bending round and terminating their action on the upper surface of the hemisphere, and others meeting, as it were, above in their progress outwards, uniting their forces to give an increased charge in the carrier ball, at an *increased distance* from the source of power, and influencing each other so as to cause a second flexure in the contrary direction from the first one. All this appears to me to prove that the whole action is one of contiguous particles, related to each other, not merely in the lines which they may be conceived to form through the dielectric, between the inductive and the inductive surfaces, but in other lateral directions also. It is this which gives the effect equivalent to lateral repulsion or expansion in the lines of force I have spoken of, and enables induction to turn a corner (1304.). The power, instead of being like that of gravity, which relates particles together through straight lines, whatever other particles may be between them, is more analogous to that of a series of magnetic needles, or to the condition of the particles considered as forming the whole of a straight or a curved magnet. So that in whatever way I view it, and with great suspicion of the influence of favourite notions over myself, I cannot perceive how the ordinary theory of induction can be a correct representation of that great natural principle of electrical action.

* Encyclopædia Britannica, vol. vi. p. 504:

1232. I have had occasion in describing the precautions necessary in the use of the inductive apparatus, to refer to one founded on induction in curved lines (1203.); and after the experiments already described, it will easily be seen how great an influence the shell-lac stem may exert upon the charge of the carrier ball when applied to the apparatus (1218.), unless that precaution be attended to.

1233. I think it expedient, next in the course of these experimental researches, to describe some effects due to *conduction*, obtained with such bodies as glass, lac, sulphur, &c., which had not been anticipated. Being understood, they will make us acquainted with certain precautions necessary in investigating the great question of specific inductive capacity.

(To be continued.)

II. *On the decomposition of water by the agency of growing plants, more particularly the Aquatic Confervæ, the Lemna, a genus of the Monœcia Diandria class, &c. &c.*
By W. H. WEEKES, Esq., Surgeon, Lecturer on Philosophical and Operative Chemistry, &c. &c. &c.

Since that period when the justly revered names of Priestly and Ingenhouse shed a halo of refulgence around experimental philosophy, and the former made known the result of his celebrated enquiries on the *respiration* of plants, not only botanists and vegetable physiologists, but chemical philosophers also, appear to have concurred in the general opinion, that plants absorb carbonic acid from the air under certain circumstances, and emit oxygen in return; and Dr. Ingenhouse concludes that this change occurs only during exposure to the direct rays of the sun. It is further presumed that in the *dark* an opposite effect obtains, and that carbonic acid gas is neither absorbed nor oxygen gas evolved; but on the contrary, oxygen disappears, and carbonic acid is disengaged.

I am neither prepared nor disposed to deny, that, "under certain circumstances," these conclusions do appear to be borne out and established, generally, by attentive observation and experiment; but there are likewise facts and circumstances, which I shall submit, warranting the conclusion that these results do not invariably obtain from the functional exercise of every description of plants, and which, I think, also render it worth while to enquire whether, as is generally as-

sumed, it be a fact that the oxygen evolved by the respiratory action in plants, is uniformly derived from the decomposition of the carbonic acid gas, as absorbed from the atmosphere, soil, &c., or from that of the more abundant source, water, in which oxygen is known to form a large proportional constituent.

A series of cautious and minutely observed experiments occupying my attention at frequent intervals during some eight or ten years past, have, I presume, authorized me to indulge in the above conclusions, and to assert that pure oxygen alone is constantly evolved, by certain plants at least, whether they be exposed to the influence of solar light alone, or subjected to the alternate changes of day and night.

The discovery of this interesting and additional feature in the operative chemistry of nature, owes its remote origin to circumstances which I feel claim from me, at least, the tribute of a brief recital. It is now about twelve years since I had the peculiar satisfaction of acquiring the scientific acquaintance and ultimate friendship of Thomas Pine, Esq., of Maidstone, in Kent, the author of a theory appropriately denominated by him *Electro-Vegetation*, the legitimate offspring of long patient observation and inductive experiment; and which theory I can have no hesitation in believing must eventually take its place among the established truths of philosophy. Immediately upon our acquaintance Mr. Pine suggested to my management a series of experimental researches such as I might conceive best calculated to subject his opinions to the severest tests of chemical and general examination. In further relation to the theory above mentioned, it is only necessary for me in this place to observe, that the conclusions of its author were amply supported by the long series of experiments in question.

During the progress of these enquiries incidental to the Spring and Summer seasons of the years 1833-4 and 5, it became expedient for me to adopt means whereby I might bring the extreme branches of various *growing* plants and shrubs into operation under a pneumatic apparatus; sometimes employing in my manipulations merely a valve of mercury with a common atmosphere in the receiver above the fluid metal, and at others causing the branches to grow for many days and even weeks within an entire atmosphere of water, limited only by the capacity of the receiver, with a view to collect and examine the gaseous results obtained during the progress of a vigorous state of vegetation. While conducting these researches by means of the usual water

trough and graduated bell glass, I often became forcibly impressed, after attentive observation, with the idea that the leaves and branches of plants, growing within my hydro-pneumatic apparatus, were materially indebted for the *large quantity* of their gaseous products to decomposition of a portion of the surrounding atmosphere of water, as well as to exhalations from the surface of their leaves, &c., originating in the decomposition of carbonic acid, though, neither then nor subsequently have I found cause to regard the generally received opinion on this subject as being devoid of foundation under ordinary circumstances and in a dry atmosphere.

Pursuing, at the period above mentioned, the train of thought thus suggested, I was led to consider the well known experiment of placing a fresh *detached* sprig of mint or other succulent plant within an inverted glass jar of water, for the purpose of exhibiting the evolution of bubbles of oxygen from the surface of its leaves exposed to the action of solar light; nor could I long hesitate in adopting, as an *opinion* at least, that the oxygen obtained in this experiment owed its origin, in no inconsiderable degree, also to decomposition of a portion of the water employed, and not entirely, as generally believed, to the carbonic acid held in solution by the fluid.

A multiplicity of engagements continued to delay my intention of endeavouring to illustrate this important question by a further appeal to experiment, until the immediate ardour arising out of the subject had somewhat abated; when, early in the Autumn of 1837, my attention thereto was strongly revived by the accidental circumstance of a decanter of river water, in which some small portion of a very minute species of *confervæ* had luxuriantly vegetated, having been left unmolested and exposed at times, during several weeks, to a strong sunlight in the window of my bed-room. I now observed that on the sides and neck of the glass innumerable bubbles of gas were collected and continuously arose from the surface of the *confervæ* or green vegetable matter before mentioned; and this gaseous product I had every reason, short of actual testing, to consider as oxygen derived from partial decomposition of the water in my decanter.

The season had too far advanced to permit of resuming my former researches, especially with the delicate *confervæ*, now the more immediate object of my attention; I therefore waited, with no small degree of impatience, the arrival of the spring and summer of 1838, with the design of subjecting my theoretical conclusions to the test of actual experiment. It was not, however, until the commencement of the month of August, that the *Punctalis*, a minute species of *confervæ* abound-

ing in stagnant waters, as ponds, ditches, water-tanks, &c., appeared sufficiently luxuriant for my purpose; when the simple but completely efficient form of apparatus, represented in the annexed sketch, was immediately put in requisition for the occasion. The bolt-head *a*, holding one gallon, having been taken to a water-tank in which a sufficient quantity of the *confervæ* in question was discovered to have vegetated, the globular part of the glass vessel was forcibly immersed beneath the surface of the fluid, until the orifice of the neck could be



brought into an appropriate position to admit of the globe filling freely, while the current of water, during its downward impetus, carried with it an ample quantity of the plant sought to be operated upon. The position of the bolt-head having been now reversed with the opening of the neck downwards, the stoneware jar *b*, charged also with the water of the tank, was plunged perpendicularly underneath, and when the neck of the glass had been immersed within the water of the jar, until the inferior circumference of the globular part rested upon the substantial rim of the lower vessel, the two were carefully removed and placed immediately in an eligible situation in my garden, subject to frequent observation. I do not think I can convey to those whom it may possibly interest, a better idea of the subsequent progress of my experiment, than by subjoining occasional extracts from my daily Journal of Memoranda, or rough notes, made on the spot at the moment of observation.

August 12th, 1838.—At 8 A. M. placed a quantity of the *Confervæ Punctalis*, in rain water, under the pneumatic apparatus in garden—evening bright with light breezes from S. W. Since being at rest the whole of the minute plant has resumed its natural tendency to the surface of the fluid, and occupies the zenith portion of the glass globe. Appearance perfectly healthy.

13th, eight o'clock, A. M.—A gas has been abundantly evolved, *during the past night*. The confervæ has been in consequence depressed from its original position in the upper hemisphere of the globe, its former place being now occupied by the gaseous product, fully equal in amount to the bulk of plant; I presume not less than from sixteen to eighteen cubic inches. The water of the jar has overflowed in a corresponding degree.

18th.—The formation of gaseous matter from *confervæ punctalis* has been almost regularly progressive during the last five days, and now occupies nearly one-sixth of the globular hemisphere. Appearances indicate that the evolution of gas is on the decline; the plant also shows symptoms of decreasing vigour.

21st.—Gas has not materially augmented to-day; and I conclude that the *confervæ* has nearly ceased to vegetate.

Four o'clock, P. M.—Resolved to transfer the gas collected from glass globe to a series of air jars of different capacities, for the purpose of chemical examination.—Six o'clock, P. M. Gas generated within the space of nine days, amounts to fifty-eight cubic inches, and proves to be oxygen more pure than usually obtained artificially. An extinguished wax taper is instantaneously relighted by being plunged therein, and phosphorus, iron wire, and other combustibles, burn with great brilliancy. A cubic inch tube, graduated in hundredths, charged with the gas and placed over pure liquor potassa.

22d. evening.—The volume of gas in cubic inch tube over liquor potassa (temperature considered) has diminished only one and a half per cent in about twenty-four hours, and has remained without decrease during the greater part of that time; consequently, the oxygen thus obtained, holds in admixture a smaller proportion of carbonic acid than that generally procured by exposing to a red heat the peroxide of manganese.

These experiments with the *confervæ punctalis* were several times repeated during the months of August and September, and invariably with similar results, as witnessed by divers scientific friends who obligingly interested themselves on the occasion. Subsequently the *lemna*, a genus of the monœcia

diandria class, (commonly denominated *duck-weed*) was often subjected to the same process of examination, with the only difference that it did not produce oxygen quite so abundantly as the *confervæ punctalis*, though the gas obtained from the *lemna* was found to be in a trifling degree more pure than that evolved by the former description of plant, inasmuch as it was proved by chemical analysis to contain only *one* per cent of carbonic acid.

In the early part of the month of October, I commenced a series of similar trials with several larger species of aquatic plants, but, owing to removal from their native habitudes at a late period of the season, my efforts were not attended with a like degree of success. From these latter experiments, however, I learned that it is only in the *perfectly healthy and vigorous state* that plants possess the power to decompose water and liberate its oxygen. Under certain circumstances, which require further researches to define, I am convinced that some plants evolve a portion of nitrogen.

As connected with and arising out of the subject of this paper, I shall permit myself to subjoin a few desultory observations. It has been ingeniously suggested to me by a highly esteemed scientific friend, that the oxygen obtained during the experiments above detailed, might possibly arise from the decomposition of a considerable portion of carbonic acid, not unfrequently held in solution by certain waters. Now, the water employed in my experiments, as first stated, was taken from a large open tank on the premises, and it is quite fair to say that few specimens could be furnished in greater purity. The substances known generally to be held in solution by rain water, are air, carbonic acid, carbonate of lime, and, according to Bergman, occasionally some traces of nitric acid and a little muriate of lime. The best authorities agree that the quantity of air in good water, of the kind in question, does not exceed one-twenty-eighth of the bulk; and that one hundred cubic inches contain generally about one cubic inch of carbonic acid gas, but I have satisfied myself that the rain water actually employed, yielded rather short of the usual assumption. If we reckon the bolt-head to contain one gallon, or about 278 cubic inches, which will be a sufficient approximation to the fact, we shall perceive that the full amount of carbonic acid in the water of my experiment could not exceed three cubic inches, a quantity quite inadequate to furnish on decomposition even a sixth part of the oxygen evolved during the first night, and the possibility of the water acquiring any addition of carbonic acid by absorption from the surrounding atmosphere, was effectually provided against.

in the construction of the apparatus. Nor does it appear that the common air contained in the water used, had been expelled and thus augmented the volume of gas ultimately measured, because the only deviation from the pure oxygen found on analysis, was from one to one and a half per cent of carbonic acid. If we suppose the plant capable of decomposing atmospheric air, a considerable quantity of nitrogen must have been manifest on examination.

Sir H. Davy, at page 192, fourth edition of his "Last Days of a Philosopher," says "those fishes that spawn in Spring or the beginning of Summer, and which inhabit deep and still waters, as the carp, bream, pike, tench, &c., deposit their eggs upon aquatic vegetables, which, by the influence of the solar light, constantly preserve the water in a state of aëration." Though the form of expression used by our celebrated philosopher is not definitive, I think I may safely assert that the means employed by nature to effect the important object alluded to by Davy, that of preserving the water in a state of aëration, consists in the power of growing plants to decompose that fluid and supply a vivifying principle to the eggs by the disengagement of oxygen. Upon similar grounds, I presume, we might fairly conclude, that the baneful influence of malaria arising from the stagnant waters of marshy districts, is, during the spring and summer materially modified by the oxygen (emphatically characterized as *vital* air by Dr. Priestly) generated from the action of confervæ and other aquatic vegetables, abundantly inhabiting the still waters of such localities. In the season of autumn, when the vigorous action of vegetation has ceased, and the plants themselves in many instances pass into decomposition, experience shows that the demon malaria begins to diffuse its most pestiferous exhalations.

It being obvious, from the experiments above recorded, that the leaves of plants are furnished with organs suited to the office of decomposing water, and as we find only one of the elements of this fluid set at liberty, it follows logically that the other element, *viz.* the hydrogen, is absorbed by the plant and adapted to the purposes of the vegetable economy; at least, I presume that I have brought sufficient evidence to show, that in addition to the offices of the roots, leaves, "*common*" and "*proper vessels*," hitherto known, nature has provided plants with another important source of action, by the *direct* exercise of which they derive from one of the elements of water, a principal constituent* of their own, while

* Some ten or twelve years since, while engaged in the analysis of upwards of forty specimens of indigenous and foreign woods, by

from the disengagement of the other, they silently administer to the purity of the atmosphere and the economy of *animal* life. I have not unfrequently spent many hours, aided by the microscope, in watching, particularly in bright days, the evolution of gas bubbles as they are formed and disengaged from small aquatic vegetables, as well as from the detached leaves of other plants immersed in water; and, as one of the fruits gathered from such observation, I imagine I shall not risk any very serious condemnation, in venturing to conjecture that the spinous or downy points presented by the superficies of leaves (and I find it is to *these points* that the bubbles of gas are invariably attracted) are analogous to so many galvanic poles, rendered more or less potent by the agency of solar light and other circumstances; thus, however minute and trivial in their individual operation, producing by their infinitude an amazing aggregate of electro-chemical action; and though, doubtless, this be most conspicuous where exercised in the stagnant pool, or meandering rivulet, yet, nevertheless, extending its natural magic equally to decompose the beautiful leaflet gem exhibited in the spangling dew-drop.

the process of close distillation, I became forcibly struck with the proportionately large volume of hydrogen frequently evolved in combination with carbon, &c.; so abundant, indeed, that I was induced to convey it into a temporary reservoir, and occasionally appropriated it as a means of illumination in my laboratory operations. If, during the growth of plants, this quantity of hydrogen be not materially derived from the decomposition of water, by a direct exercise of their external functions, it will be extremely difficult to account for its origin and presence as a component of ligneous fibre; for, though we are not yet accurately familiarized with the *internal* organization of vegetables, and their consequent capabilities, it seems scarcely probable that to this single and somewhat limited source alone, the whole of the hydrogen is attributable which we find resulting on careful analysis.

III. *Experimental and Theoretical Researches in Electricity. Second Memoir.* By WILLIAM STURGEON, Lecturer on Experimental Philosophy at the Hon. East India Company's Military Seminary, Addiscombe, &c.*

Read March 3d, and December 19th, 1838.

On the Identity or Non-identity of Electricity and Magnetism—Different opinions of Philosophers on this topic—Experimental Examination of those Phenomena which are supposed to favour the hypothesis—Examination of M. Ampere's Hypothesis—The polar forces of hard steel Magnets unvanquishable by Electric Currents—The inefficiency of Electric Currents in magnetizing hard steel to a high degree of power—The distribution of magnetic force exhibited by Steel Magnets and by Loadstone, not imitable by Electric Currents.

88. In the first memoir which I had the honour to present to this Society, I endeavoured to elucidate those fundamental principles of electricity, which appear obviously developed by an extensive series of illustrative phenomena, and well calculated to afford an easy explanation of the nature and peculiarity of electric action. There still, however, remains one very important theoretical point on which I have not yet touched; a point which is yet wavering under the dominion of vacillating opinion, without any party venturing a demonstration of his peculiar ideas: or, indeed, showing much, if any, reason for entertaining them.

89. The discovery of the identity of lightning and ordinary electric discharges, by Franklin, and the well established facts of lightning depolarizing compass needles, reversing the polarity of others, and producing other remarkable magnetic phenomena, were events that have, long ago, led philosophers to imagine that electricity and magnetism are not distinct powers of nature: but that, more probably, they emanate, in different forms, from one and the same physical cause. The apparent similarity of the attractions and repulsions in magnetism and electricity, has also been considered as favourable to the hypothesis.

90. It is now more than half a century ago since the celebrated Father Beccaria ventured an opinion, that the electrical and magnetic powers are identical. "Are not these peculiar effects of the electric fire with respect to magnetism,"

* From the Transactions of the London Electrical Society.

said this eminent philosopher, "so many proofs which corroborate my former conjectures, that the peculiar magnetic force observed in *loadstone* is to be attributed to either atmospheric or subterraneous strokes of lightning; and that the *universal systematic* properties of magnetic bodies are produced by an universal systematic circulation of the electric element?"* This hypothesis of the illustrious Italian was not much attended to, till the discovery of electro-magnetism, which happened nearly fifty years afterwards; when it was again broached, as a new idea, by M. Ampere. Since that time the hypothesis has gained many proselytes, though there be still some philosophers who do not entertain that opinion: and as electricity has latterly produced many phenomena, whose true cause can only be understood by a proper solution of the problem which this disputed point has created, a strict investigation of the various circumstances connected with it can hardly fail to be interesting to the Electrical Society: I have therefore devoted the whole of this memoir to that particularly important subject, in which, it will be found, I have collected, examined, and arranged the most striking instances of analogy in electricity and magnetism: and have also pointed out many phenomena in which they as obviously disagree. I have contemplated the whole as profoundly as I have been able, and have discussed the various topics as I have proceeded, with freedom and candour, in the manner following:—

91. If one of the poles of each of two magnets be presented to each other, a tendency either to recede from, or approach each other is immediately manifested, accordingly as these poles are similar or dissimilar respectively; and because similar and dissimilar electrized bodies evince corresponding tendencies to move *from* or *towards* each other, the two sets of phenomena have been regarded as marking a strong analogy, and have been held forth as evidence in favour of the identity of the magnetic and electric agents. But, before these, or any other supposed analogies be permitted to enter into any code of physical laws, they ought to be examined with the most rigid scrutiny and exactness. The phenomena ought not only to be compared with each other, but each individual event should be traced, as closely as circumstances will permit, to the nearest cause of its production; and in what manner it would be affected by varying the conditions of the experiment: and, in the question before us, it is only from such close investigations as these, that data are to be obtained which can be esteemed of much intrinsic value.

* Treatise on Artificial Electricity. By Father Giambatista Beccaria, p. 310, English edition, London, 1776.

92. In contemplating the phenomena I have been speaking of in the manner proposed, let it be supposed that $ns\ s'n'$, fig. 1, Plate II, are two magnetic needles, each suspended by a fine thread; and that p and n , fig. 2, are two dissimilarly electrized balls, suspended in a like manner. Then, because of the magnetic poles $ns\ n's$, which are opposite to each other, being of different kinds, they will approach each other until they come into contact: and a parallel phenomenon will be exhibited by the dissimilarly electrized balls, $p. n$. Thus far the analogy appears to hold good. Our conclusions, however, are not to be drawn from these facts alone, for the motions already performed are the mere preliminaries to the display of other phenomena which demand still greater attention, and reveal the operation of other attributes than those which brought the bodies together. The electric balls, p, n , very shortly after the first contact, separate from each other; and if their first electric conditions were of equal degrees *above* and *below* the common standard, or neutral state, they would *neutralize* each other's action, and their fibres of suspension would hang parallel to each other. But if their first electric conditions were not of equal degrees above and below the natural standard, both balls would remain either *positively* or *negatively* electrical, accordingly as p or n exhibited the greater degree of electric tension prior to the first contact. In either case the balls would display a tendency to recede from each other, and diverge their fibres of support.

93. Now the motions last exhibited by the electric balls find no parallel phenomena in the magnetic poles $ns\ ns'$, fig. 1, which still cling together without evincing the least tendency to separate: instead of which, it is a well-known fact, that the longer those poles are permitted to remain unmolested the greater degree of force would be required to separate them. Hence, then, without entering into any theoretical disquisition, these electric and magnetic phenomena are so obviously dissimilar, that instead of being susceptible of inferences in favour of an identity in the operating causes, they have an obvious tendency to bias the mind to the very opposite conclusion.

94. Let the two electric balls, p, n , fig. 3, be suspended on the opposite sides of a fixed ball B, which is in the natural electric condition. The electric bodies p and n will immediately approach B; and after contact with that body they will recede from it. When the body B is insulated, and the bodies p and n differ in degree of electric tension, *above* and *below* the natural standard respectively, all the three bodies remain electrized after contact: and p and n exhibit a ten-

dency to recede from B. If, on the other hand, p and n are of equal degrees of electric tension *above* and *below* the natural standard, they will neutralize each other through the medium of B; and B also will remain neutral. If the body B were uninsulated, it would be a matter of no consequence in what manner p and n were electrized, they would both become neutralized by contact with that body. Here then we have three conditions under which the electric balls, p and n , would approach B by electric action; but in no case would they be retained in contact with that body. In every variation of these experiments the bodies, p and n , would have their electric energies considerably deteriorated by contact with B; and in some cases those energies would totally vanish by such contact, however powerfully they might previously have been displayed.

95. Let now a parallel experiment be made in magnetics, by suspending two light bar-magnets by threads as represented by fig. 4. When the inferior dissimilar poles n s' hang on the opposite sides of a soft iron ball i , as in the figure, they immediately approach that ball; and when they have once come into contact with it they remain attached to it; and the longer they are left undisturbed the greater is their tendency to remain there: so that the contact, instead of diminishing the attractive force, absolutely increases it. How very different are these events to those which occur by electric action. In every case of contact by magnetic attraction, the forces which bring the bodies together, become exalted in some proportion to the closeness of contact: and in no case are those forces impaired by *time*. The electric attractive forces, on the contrary, are invariably, and immediately impaired by the bodies touching one another. In some cases they are suddenly and totally neutralized; and in no instance are they of long duration independently of a continuous exciting process.

96. Electro-polarization (52,) has an apparent analogy in magnetism, but the different ways in which the experiments may be varied, lead to results which show an obvious difference in the causes producing them. The nearest responsive fact is the polarization of soft iron by placing it in the vicinity of a permanent magnetic pole. If, for instance, the piece of soft iron s' , n' , fig. 5, be placed near to the magnetic pole s , of the steel bar s , n , a magnetic polarity will immediately be displayed in the iron bar: and arranged as indicated by the letters, viz. the south pole s of the magnet s , s , will cause a north pole in the vicinal extremity n' , and a south pole in the remote extremity s' of the iron bar: but if the north pole of the magnet be presented to the soft iron as represented by

fig. 6, the order of polarity in the iron will be the reverse of that in the former instance : though still in accordance with the same law : for in both cases the poles in the permanent magnet occasion poles of the opposite kind to be exhibited in the nearest extremity of the iron : and polarity of the *same* kind in the remote extremities of the iron.

97. The circumstances under which the magnetic polarity thus displayed by pieces of soft iron bears so strong a resemblance to those necessary to the production of electro-polarity (62, figs. 89 and 90, Pl. XII, Vol. 2,) that a superficial observer might easily be led to imagine that the same agency was in operation in both cases : but here, as in the cases already described (92, 93, 94, 95,) a close investigation of these phenomena, and a correct view of those which a variation of the circumstances productive of them exhibit, lead to very different inferences. Let us, for instance, permit the pieces of soft iron, as in figs. 5 and 6, to touch the permanent magnetic poles to which they are presented. The steel and iron would remain as decidedly polar as before : and the remote poles s' and n' of the two pieces of iron, and n and s of the steel bars would display still stronger polar forces than prior to the contact. These facts have no parallel in electricity : for if the electric bodies P and N, figs. 89 and 90, Pl. XII, Vol. 2, be brought into contact with the bodies n , p , and p , n , to which they are respectively presented, the phenomena of polarity cease to be exhibited : each pair of bodies immediately becomes similarly electric throughout ; the one pair, fig. 89, being all in an electro-positive condition, and the pair, fig. 90, being in an electro-negative condition, on every part of their surfaces.

98. The electric phenomena displayed by bringing the bodies P, and n , p , fig. 89 ; and N, and p , n , fig. 90, are easily explained by supposing an introgression of fluid from the relatively positive to the relatively negative bodies of each pair : but it would be exceedingly difficult to understand how the magnetic bodies maintained their polarity by any *similar* distribution of a fluid, or of any other physical agent, for whatever may be the nature of the magnetic agent, it is obviously more determinedly fixed or accumulated in the extremities of ferruginous bars by close contact, than when those bodies are at an appreciable distance from one another. Hence we discover that the magnetic and electric forces, which, at certain distances, effect such a similarity of phenomena in bodies situated in their respective localities, are productive of no corresponding facts when the approximation of those bodies is sufficiently close. Neither do the phenomena agree which the newly magnetized and electrized bodies

exhibit after they have quitted those original magnetic and electric bodies whereon the respective disturbing forces reside ; for, after the separation of n, p , and p, n , figs. 89 and 90, Plate XII, Vol. 2, from P and N respectively, the former would exhibit *positive* and the latter *negative* electric action : but the pieces of iron, figs. 5 and 6, Plate II, would lose all traces of magnetic action, when once they were sufficiently removed from the localities of the magnets to which they had been attached.

99. If it can be imagined that by substituting steel for the pieces of soft iron in figs. 5 and 6, Plate II, an analogy to the phenomena exhibited by the electrized bodies would have been more apparent, by the steel retaining magnetic action after quitting the disturbing magnetic poles, I would observe that, its retaining some trace of magnetic action is a fact which cannot be denied : but in that case the steel would remain polar, as is always the case with magnetic bodies : and as no trace of polarity would be exhibited by the electric bodies, but on the contrary, an uniformity of electric action would be discoverable over every part of their respective surfaces, the *supposed* analogy again loses its support, and as decidedly fails in this instance as in those previously discussed. Moreover, the pieces of steel would retain their polarity unimpaired, even after long continued contact with other bodies ; whereas the electric bodies would lose all trace of electric action by the slightest touch with uninsulated conductors.

100. A globe of steel may be made to exhibit *permanent* magnetic polarity when far removed from every disturbing force : but the same globe will not maintain any corresponding electric action. A plate of glass will exhibit electro-polarity, on its opposite surfaces, for some considerable time after it has been removed from the exciting apparatus : but magnetic polarity is not known to be exhibited by glass. If then the magnetic and electric elements be identical, why this capricious selection of bodies for the display of these parallel phenomena ? The electric forces will attract all kinds of matter without exception ; but the magnetic forces appear to be exceedingly select in this particular ; operating on particular kinds only. Coated glass, whatever may be its form, affords no *permanent* electric attractions, which are, in the least, comparable with the attractions exhibited by magnetic bodies : for if a metallic arc connect the two sides of a Leyden jar, the electric forces immediately disappear ; but an iron arc connecting the poles of a horse-shoe magnet is permanently held there, unless removed by mechanical violence ; and the longer it remains undisturbed by extrinsic force, the more vigorously is it

attracted by the poles ; and there is no known substance whatever, by which the poles of a magnet may be connected, that will, in the least, deteriorate their powers.

101. Those few kinds of elementary matter on which magnetic attractions are known to be exerted, display no distinction of respect for the *north* or *south* polar forces, being attracted indiscriminately, and to the same extent, by both. Very different indeed are the nice discriminations of the *positive* and *negative* electric forces manifested in an almost endless variety of phenomena, every one of which teems with interest in the contemplations of the philosopher, and beautifully characterizes the agency of their production. If, for instance, an intimate mixture of sulphur and red lead be indiscriminately projected through the air to a series of *positively* and *negatively* electrized surfaces, the powders will be separated from each other by the dissimilar electric forces, into whose spheres of action they are thrown ; and the sulphur and red lead will respectively be found at the positive and negative surfaces, exhibiting a peculiarity of arrangement not known to be accomplished by any other kind of physical agency.* Similar selections are uniformly exhibited by electric forces, whenever the particles of compounds on which they operate are sufficiently voluble to be put into motion by them, or are held together by inferior powers. Every individual electro-chemical decomposition appears to be an instance of this kind of action, and demonstrates the peculiarity of this important fact.

102. It has been said by M. CErsted, that the only difference in the electric and magnetic forces rests in their different degrees of tension or activity ; the electric being the more active or vigorous in its operations : and this hypothesis has been attempted to be supported by M. Ampere and other philosophers, whose opinions on this subject will long command respect. But I must confess that I can discern no satisfactory discrimination of this kind, nor am I acquainted with any facts that are even in the least favourable to it. It is well known that electric attractions are the most powerful when the bodies exhibiting them manifest the greatest degree of tension in the display of all other electric phenomena. The spark, for instance, is shown to the best advantage when the

* This fact was first shown by Leightenberg. Cavallo and Bennet, especially the latter philosopher, have extended the original experiments of Leightenberg, and varied them in a variety of pleasing and interesting ways.—*Bennet's New Experiments on Electricity.* Derby, 1789.

electric body, whence it proceed, exhibits the greatest degree of attraction: and the charge of a jar is accomplished in the shortest period of time, and with the greatest degree of facility, under similar circumstances. Moreover, when electric discharges are performed, either from a single jar, or from a battery of jars, the striking distance is greatest, the flash is the most brilliant, the noise is the loudest, the physiological effects are the most powerful, and, in fact, every phenomenon is exhibited under the most advantageous circumstances, and in the most perfect manner, when the jar, or battery, is in the most suitable condition for a display of its attractive energies.

103. But now let us enquire into the *extent* to which electric attractions are usually exhibited. Has any electrician ever seen a prime conductor, (which always shows attraction more powerfully than any other electric apparatus) support, by its electric energies alone, a single *ounce* of any kind of matter? I presume not. If, then, with this insignificant attracting force, electricity be prepared for a display of some of its most splendid and terrific phenomena—the production of vivid light, intense heat, the noise of thunder, and the destruction of animal life: and that magnetism proceeds from the same cause or agency, it seems natural to ask, why it is that similar phenomena are not exhibited to the same, or even a greater extent, by a magnetized body whose attractions are ten thousand times ten thousand greater than any ever witnessed in electricity? These important questions, which stand so prominently and essentially in the path of investigation, demand the most profound contemplation of the philosopher, and must not be passed over in silence by those who are endeavouring to identify the electric and magnetic powers. We have yet to learn the mode of producing a *magnetic spark*, and are totally ignorant of the sensation communicated by a *magnetic shock*. And *magnetic chemistry* is so profoundly obscured from our knowledge, that no one knows even of its existence.

104. If our reasoning be permitted to rest on facts alone, independently of favourite notions and ingenious hypotheses, which are but too apt to captivate the imagination of the superficial observer, and, sometimes, even to sap the understanding of the more studious in science, the obvious contrasts in the phenomena presented by electricity and magnetism enforce themselves upon our notice too powerfully to be misunderstood. Even the attractions, themselves, in which *alone* the appearance of analogy exists, are so exceedingly dissimilar, so truly distinct from one another, that their peculiar characteristics are well defined and easily discernible, and

cannot be mistaken by those who devote to them a proper and sufficient degree of attention.

105. An insulated electrized globular body *radiates* its attracting influence on every side alike, when surrounded by an uniform medium, such as the atmospheric air, as may be understood by fig. 7, Pl. 2, which may represent a great circle of the globe with its radiating electric force. But a magnetized globe, similarly situated in space, exhibits no such radial influence; for being polar on opposite points (*n. s.* fig. 8,) of its surface, the greatest *disposable** attracting forces are exerted about those polar regions, and especially in the line of their axis continued. At right angles to that axis, in the plane of the equator, *e e*, the polar forces, by their mutual attractions, nearly balance one another; neither of them exhibit-

* It appears by the distribution of iron-filings, when strewed on paper, above a bar magnet, that a considerable portion of the *north* and *south* forces are engaged in attracting one another, as shown by the curve lines assumed by the filings; and, consequently, are not employed, or, at least, very sparingly so, in any attractions which the magnet exercises on foreign bodies, such as pieces of soft iron, magnetic needles, &c., placed a few inches distant from its extremities and in a line with its axis; or, indeed, opposite to any other part of its surface; and, although much more of the magnetic force is brought into play as the iron is brought nearer, and most of all when it is in contact with the pole of the magnet, there is still a considerable portion of force which cannot be exerted on this foreign body, because of its being engaged with the opposite force, about the surface of the steel, which lies between its extremities; and especially that which is situated near to its centre. For convenience then, I call that portion of the magnetic force which lies about the equatorial part, the *engaged force*; and that which is brought into play on foreign bodies, the *disposable force*.

The *disposable force* of any magnet may be diverted from its original directions of action by the approximation of ferruginous bodies; and, in some instances, nearly the whole of it may be drawn from a body on which it operates, without moving either the magnet or the body. To illustrate this point, let a bar magnet be placed six or eight inches distant from the pivot of the needle, and at right angles to its direction. The *disposable force* of the magnet will deflect the needle to some considerable number of degrees. Now place on each side of the magnet, parallel to it, and about three inches distant from it, a piece of soft iron, about its own shape and size. The deflection of the needle will lessen considerably, showing that a portion of the *disposable force* has been diverted from its action on the needle. Now, bring the pieces of iron nearer to the magnet, and the deflection again decreases; and when the pieces of iron are brought into close contact with the magnet, one on each

ing much *disposable* influence on exterior bodies. Another great characteristic distinction in the display of the electric and magnetic forces by these bodies appears to be this;—the electric force is wholly *disposable* and ready to be exerted upon, and even *transferred* to, other vicinal bodies: whereas the magnetic forces are neither *transferable* nor wholly *disposable*, for no magnet has yet been known to have its power impaired by contact with unmagnetized bodies, and in no case is the whole of its attracting power exerted upon a vicinal body.

106. I have been exceedingly anxious to discover, if possible, some facts which might afford analogies whereon to fix a basis of reasoning on the identity of these physical agents; but, although I have met with some further phenomena, far from being uninteresting in the discussion, a close examination of their true character has shown their evidence in favour of the supposed identity to be of no more value than that afforded by the facts already noticed.

107. If there be one electric apparatus more than another, whose action resembles the action of the magnet, it is the dry *electric column*, whose polar forces are more uniformly and permanently exhibited than those of any other electrical instrument. But the attractive and repulsive powers of this instrument, like those in all other electrical arrangements, are exceedingly feeble when compared with the gigantic powers of a magnet; they are, moreover, directed towards, and operate upon, every kind of matter without distinction, whereas the magnetic attractions and repulsions, notwithstanding their vigorous action on ferruginous bodies, are, with the exception of one or two of the metals, perfectly inert on all other kinds of matter. The attractions and repulsions of the electric column are productive of vibratory motions in pendulous bodies properly situated between the poles; which

side, from end to end, nearly the whole of the *disposable* force will be exerted on the iron, and but very little of it, if any, will reach the needle so as to cause a perceptible deflection. Now, in this case, the extremities of the magnet are still untouched by the iron, and are, consequently, as much exposed to the needle as when the iron was not present; notwithstanding which, it is obvious from the experiment, that the *disposable* force which before deflected the needle has now taken another direction, and is employed in polarizing the pieces of soft iron. The disposable force of the magnet, however, although it cannot now reach the needle with a sufficient degree of formidableness to accomplish deflection, is not entirely engaged by the iron, a residuum still remaining, which is detected by bringing the needle nearer to the magnet.

show that the vibrating body changes its electric condition at every contact with either pole of the instrument, and accommodates itself to the attractive influence of the opposite pole. When the pendulous body has come into contact with the positive pole, it acquires an electro-positive condition, and is repelled to the negative pole, where it deposits its charge and becomes electro-negative. It is now again under the attractive influence of the positive pole, to which it is compelled to make another journey, and *from* which it receives a new charge and an immediate succeeding repellent impulse, which again directs it to the negative pole; and in this manner the suspended body performs its vibratory motions, being in an electro-positive condition whilst travelling in one direction, and in an electro-negative condition whilst travelling in the other. By these means a *pulsatory current* permeates the pile from the negative to the positive pole, the fluid being transported through the air, from the latter to the former by means of the pendulous body.*

108. Besides the pendulous motions already alluded to, the dry electric column is productive of physiological and chemical phenomena, will emit sparks and charge coated glass and other inferior conductors, as decidedly as charges are produced by the machine: all of which are so perfectly distinct from, so decidedly foreign to, any known capabilities of the magnet, that there is not to be found one solitary trace of analogy in the performance of the two kinds of apparatus. The attractions and repulsions are the only phenomena in which there is a *shadow* of resemblance, whilst in *reality* even this faint analogy has obviously no special existence. The delicate electric forces which alternate the conditions of, and give vibratory motions to, the pendulous body, find no similarity of action in the majestic attractive forces of the magnet, which select those of their own species only; whose coeval polar affinities mutually exalt the action, and constrain the attracted body to assume a determinate polar condition, and prevent its escape from the vigorous influence of the pole to which it is first attached. Hence as no vacillancy in the magnetic condition of the attracted body is produced, the grand essential to vibratory motion has no existence in magnetics: nor can any such locomotions, as those exhibited by

* As this discussion requires experimental facts rather than theoretical opinions, I have not, in this place, entered on the doctrine of the dry electric column. It is possible I may have occasion at some other time, to enter fully into the philosophy of this interesting apparatus.

the electric column, be produced by any known self-acting powers of the magnet.

109. If we are to look for the supposed identity of electricity and magnetism amongst electro-magnetic phenomena, we are still as far from arriving at satisfactory conclusions as in any other branch of the science. It is true, we here find some of the most striking and interesting affinities which electricity and magnetism have hitherto developed; affinities which will ever link these sciences together in the firmest bonds of physical union, though by no means identifying the elements by which the phenomena are produced. Each elemental agent plays its own part in the production of electro-magnetic phenomena as decidedly as in those of magnetic electricity, whose display is accomplished by the reciprocal excitement.

110. From the attractions and repulsions exhibited by wires carrying electric currents, M. Ampere was led to imagine that all magnets owe their influence to an unremitting circulation of the electric fluid; an hypothesis so exceedingly ingenious, and so eminently calculated to favour the expectations of some philosophers, that there can be no astonishment excited by its gaining proselytes amongst those whose minds were already predisposed for its reception. But, notwithstanding the respect which is due to the talents of those philosophers who have favoured Ampere's views on this topic, I must candidly confess that the hypothesis has always appeared to me to be much easier to acknowledge than to understand. In the present investigation I have considered experimental facts as the only data on which I can proceed with any chance of success of arriving at a close approximation to true theoretical inferences. I have, therefore, neither ventured an opinion of my own, nor permitted the views of others to influence the inquiry.

111. The imaginary electric currents to which Ampere refers all magnetic action, lead us to enquire into the character and situation of their source, and by what means they can be supposed to be *perpetually* and equably maintained, either on the surface, or within the body, of a steel bar. Here it is that we are led to enumerate and examine all the known artificial sources of electric excitement, and endeavour to trace their influence to the operations of permanent steel magnets. Independently of *magnetic* excitation, we know of only three sources of electric currents, viz. frictional, voltaic, and thermal: for besides these four, there are no other sources known:* hence if a bar of steel which exhibits *permanent*

* The dry electric column is here omitted.

magnetism has that power conferred upon it by the influence of electric currents, which must necessarily be as durable as the magnetic action itself, to which of these sources are we to look for the *supposed* actuating currents? Or are there other sources of electric currents of which we are yet entirely ignorant? But, from whatever source those imaginary currents may be supposed to proceed, that source must necessarily be situated either on the surface, or within the body, of the steel. The idea of electric currents being excited by *friction* amongst the particles of the solid metal, is too absurd to be entertained for a moment: and the conditions necessarily required for the production of *voltic* currents, are no where to be found in the steel: hence our enquiries are necessarily limited to *thermal* excitation alone.

112. That thermo-electric currents are producible in every piece of metal, whether pure or compound, is a fact which I have proved by very extensive experiments, some years ago.* But it must be understood that to produce an electric current by any means whatever, requires a co-existent motion in some of the elements employed during the whole time the current is flowing: unless it be of a momentary duration only, and the effect of an impulse, in which case the current may continue to flow for a short time subsequently to the terminal exciting impulse. When a current is produced by an electric machine, the glass cylinder, or plate, as the case may be, is necessarily kept in motion. When a voltaic combination is the electric source, the *liberated* elements of the liquid in the battery are put into motion and become vehicles for the transportation of the electric fluid to and from the solid parts of the arrangement: and a thermo-electric current depends upon the motion of the calorific matter: for when that element is perfectly at rest in the combination, the electric current ceases to flow.

113. From the above considerations it appears, that a perpetual propagation of thermo-electric currents on the surface, or within the body, of a steel magnet would require a perpetual motion of caloric within its mass: which motion, unless the production of some hidden, mysterious, and unsuspected agent within the steel, would require as continual an influx and efflux of the calorific element from and to the surrounding medium. Moreover, the laws of electro-magnetism require that the direction of the electric currents should be at right angles to the axis of the steel bar; and the ingenious author of the hypothesis has ventured to assert that their route is in that direction, in a series of parallel spirals round

* Philosophical Magazine and Annals of Philosophy, vol. x. p. 1.

its surface.* Such, then, are the necessary conditions upon which Ampere's hypothesis essentially depends; and being now, probably for the first time, disrobed of their mysterious habiliments, I must necessarily resign the glory of their *discovery* to those philosophers who still entertain the idea of their existence in the steel, and who may possibly be enabled to penetrate the subject still deeper than I have investigated it. But before I quit this important topic, I will mention a few more facts, which to me, have appeared of some consequence, and can hardly fail to be interesting to others who may be induced to pursue the enquiry.

114. If the temperature of one extremity of a steel bar be elevated, and, by that process, electric currents become excited, those currents would necessarily be more powerful than any which can be supposed to exist in the metal at its natural temperature: and if the other extremity of the steel were to be heated, and again thermo-electric currents be produced in it, these latter currents would be propagated in the opposite direction to the former, and consequently the magnetic forces which they brought into play would be exerted in the reverse order to those which the first currents excited: and these artificially excited electro-magnetic forces being more powerful than any which the *supposed* natural electric currents could produce, they would predominate over these latter, and give new energies to the bar, reversing its poles in accordance with the directions of the currents. But on making the experiments, and carefully examining the phenomena, I find that no such corresponding changes have taken place in the polar forces of the magnet: and, although the poles themselves are considerably molested during the unequal temperature of the extremities and other parts of the magnet, and are removed from their original positions by the heating process, they do not assume those positions and variations of force which the thermo-electric current would necessarily give to them, were they governed by no other influence:† hence I infer, that

* *Annales de Chimie et de Physique*, t. xv.: and Ampere's *Recueil des Observations Electrodynamiques*.

† At the time this memoir was first drawn up, only a few experiments had been made on this part of the enquiry, the general results being such as are described in the text. But, whilst writing a fair copy for the press, I was led to reconsider this part of the subject, and it occurred to me, that by pursuing the experiments, some results might probably appear which would be interesting in the theory of terrestrial magnetism. I, therefore, resumed the enquiry and have been led to some novel facts which, to me, have appeared exceedingly important, by throwing a new light on the action of caloric on magnetism. They will be explained in the Third Memoir.

thermo-electric currents do not constitute the sustaining power of the magnet.

115. I next subjected a steel bar magnet to the influence of electric currents proceeding from a voltaic pair of copper and zinc. The voltaic combination was of the cylindrical shape and size, which, as is well known, I have long employed for electro-magnetic purposes, the zinc being surrounded with brown paper or calico, to prevent contact with the inside of the copper; and the whole placed in a pint porcelain jar, the exciting liquid being a solution of nitrous acid in water. The magnet which I employed was of hard-cast steel;—cylindrical, and about 6 inches long, and $\frac{3}{4}$ of an inch in diameter. It was well polished on an emery wheel, and of considerable power. It would lift, by one of its poles, a piece of soft iron of its own weight. A piece of soft iron of precisely the same figure and dimensions as the magnet, was also provided. A single helix of copper wire, No. 13, of the same length as the magnet, was formed on a hollow pasteboard cylinder, of sufficient width for the easy introduction of the magnet or iron. With these preparations, and a compass-needle furnished with an agate cap, and supported by a fine steel point, the experiments were carried on in the following manner.

116. When the meridian line of the compass-box had been adjusted parallel to the needle at rest, the helix was placed on the eastern side of its pivot, with its axis in the same horizontal plane as, and at right angles to, the axis of the needle; the nearest extremity being 12 inches from the needle's pivot. Fig. 9, Plate II, is a representation of the arrangement, where C is the compass-box, H the helix, and B the battery. Before the battery connexions were made with the helix, the magnet was introduced to the interior of the latter with its marked end nearest to the needle, consequently at 12 inches distant from its pivot. The south end of the needle was drawn towards the magnet a certain number of degrees, and this deflection being noted, the magnet was taken out of the helix, and replaced again with its poles in the reverse order, by which means the north end of the needle was drawn towards the magnet, which deflection was also noted. The magnet's action on the needle being thus ascertained, the electrical force of the battery was laid on, whilst the magnet was in the helix; and when the deflection arising from this combined force had been ascertained, the battery connexions were reversed, and consequently the direction of the current in the helix was reversed also. This last direction of the current gave a new deflection of the needle, which, after being ascertained, was also noted down. This done, the

magnet was reversed in the helix ; and when the deflections of the needle arising from the current traversing the helix in each direction respectively had been ascertained, the electric current was finally cut off, and the deflecting power of the magnet alone again ascertained in the same manner as at first.

117. The bar of soft iron was next placed in the helix, and the electric current again laid on ; and when the deflection arising from the polar force of the iron, by the first direction of the current, had been ascertained, the battery connexions were reversed, and with them, of course, the polarity of the iron was reversed also. The new deflection was noted down, and the iron finally removed from the helix. The deflecting power of the current alone, when no iron nor magnet was in the helix, was also ascertained at different times during these experiments ; two sets of which were made with two different batteries—the former by an old battery, and the latter by a new one. The results, with all the necessary particulars, are arranged in the following tables:—

FIRST SERIES OF EXPERIMENTS.

Deflections with the magnet in the helix, with and without the electric current from the old battery : and magnet retouched.

With or without the current.	Marked or unmarked end of the magnet nearest to the needle.	North or South end of the needle drawn towards the magnet.	Deflections	
Without	Marked	South	15°	1
Ditto.	Unmarked	North	16°	2
With	Marked	South	17°	3
Current reversed	ditto	ditto	7°	4
With	Unmarked	North	18°	5
Current reversed	ditto	ditto	9°	6
Magnet alone . .	Unmarked	North	13°	7
Ditto.	ditto	ditto	12°	8

118. The electro-magnetic force in the helix alone, by this battery, produced no perceptible deflection of the needle : but when the soft iron was placed in the helix, the mean of several deflections, with the currents in different directions, was 17°.

119. By taking the mean of the deflections 3 and 5 in the table, which are those obtained whilst the electro-magnetic action of the current conspired with that of the magnet, and comparing that mean (17·5°) with the mean of the deflections

with the soft iron (17°), we find that they are nearly to the same extent. And by comparing these again with deflections 1 and 2, which are due to the magnet alone, we discover that a current which is incapable of exalting the original deflecting power of the magnet 2° , is yet capable of raising a deflecting power in soft iron, equal to the whole of that exhibited by the magnet, even when aided by the influence of the current. We discover also, by deflections 4 and 6, that the same current, when exerted in *opposition* to the energies of the magnet, is incapable of counteracting more than one-half the deflecting power of the latter. And we learn, by comparing deflections 7 and 8, which are those due to the magnet after being subjected to the *reverse* electro-magnetic action of the current, with deflections 1 and 2, that the *same* electric current, which excited so great a power in soft iron, was incapable of reducing the *permanent* action of the magnet more than one-fifth of that which it originally exhibited.

SECOND SERIES OF EXPERIMENTS.

Deflections with the magnet in the helix, with and without the electric current, with the new battery; and magnet retouched.

With or without the current.	Marked or unmarked end of the magnet nearest the needle.	North or South end of the needle drawn towards the magnet.	Deflections.	
Without . . .	Marked	South	20°	1
Ditto . . .	Unmarked	North	19°	2
With current . .	Marked	South	25°	3
Ditto reversed. .	ditto	North	1°	4
Magnet alone . .	Ditto	South	11°	5
Ditto	Unmarked	North	9°	6

Magnet re-magnetized.

Without . . .	Marked	South	21°	7
Ditto . . .	Unmarked	North	21°	8
With current . .	Unmarked	Ditto	27°	9
Ditto reversed. .	Ditto	South	2°	10
Magnet alone . .	Ditto	North	8°	11
Ditto	Marked	South	10°	12

With this battery the soft iron gave a deflection of 18° ; and the current alone, without either magnet or iron in the helix, about 1° .

120. In this second series of experiments there is displayed a manifest superiority of electro-magnetic action over that

shown by the old battery; but although deflections 4 and 10, show that the electro-magnetic action completely counter-balanced the deflecting force of the steel magnet, deflections 5, 6, 11, and 12, as obviously demonstrate that the original magnetic power was very far from being annihilated, and that, notwithstanding the vigorous electric current to which the bar had been subjected, the latter retained about one half of its original power, which that current was unable to subdue. Indeed it appears from both series of experiments that a great portion of the electro-magnetism of the helix operates merely on the *disposable* part of the magnet's force, and diverts it from its original direction, in the same manner as soft iron, or other magnets would do; and the electro-magnetic force thus engaged, is prevented from assisting the other portion in conferring permanent effects on the steel. When the constraining electro-magnetic force is removed, the liberated disposable force of the magnet with which the former had been engaged, again resumes its original direction, and gives the needle a new deflection, in the *same direction*, though not to the same extent as at first (Deflections 5, 6, 11, 12.).

121. I am not aware that any one would venture to assert that electric currents, more powerful than those employed in these experiments, still existed in the steel: and if not, to what cause are we to allude the retained magnetic force? There must be some agent in operation which still sustains the polar action, and resists the energies of the assailing electric current. That agent cannot be electricity, or it would have been subdued by the counteraction of a superior electric force; it must, therefore, be admitted, that some other physical agent, perfectly distinct from the electric, presides over the polar forces of the steel magnet.

122. I am well aware that, had the electro-magnetic force of the current been more powerful, the magnetic forces of the steel would have suffered to a greater extent; and it is possible that an electro-magnetic force might be employed of sufficient extent to completely annihilate the original polarity of the steel, or even reverse its polar action; but I should wish it to be understood, that to accomplish such an effect, the electric current employed must be very powerful indeed: and whatever extent of polarity might be exhibited by the steel after the removal of the exciting electro-magnetic force, the *retention* of that polarity could not be supposed to depend upon that *absent* exciter, any more than the polarity of this, or any other piece of steel, could be supposed to be sustained by the absent magnet which first excited it: and our present know-

ledge of electro-dynamics does not permit us to indulge in the idea that any sustaining electric currents remain in the steel.

123. We have seen by the preceding experiments, that the power of the magnet was considerably lessened by the action of the electro-magnetic force in the helix; but it must be observed that the latter force had no *sustaining* power to contend with, excepting that exercised by the retention of the steel: but if the magnet be placed under the influence of a *sustaining* magnetic force during the time it is assailed by the electro-magnetism of the helix, it will be found that the latter is too impotent to make any other than a very slight permanent impression on the original power of the steel magnet; and, under some circumstances, not the slightest impression is accomplished. To prove this fact, I place the *marked* end of a magnetic bar, seventeen inches long, in contact with the *unmarked* end of the six inch cylindrical magnet whilst placed in the helix, the marked end of the latter being nearest to the needle, as represented by fig. 10, Plate II. I now transmit the electric current through the helix, in a direction which tends to neutralize the magnetism of the inclosed bar. The current is continued for more than a minute, after which it is removed, and as speedily as possible, the long sustaining magnet is removed also. This done, the deflecting power of the cylindric magnet is again ascertained. The following table shows the results.

THIRD SERIES OF EXPERIMENTS.

Deflections with the magnet in the helix, with or without the electric current, from a new battery.

With or without the electric current.	Marked or unmarked pole of the magnet nearest to the needle.	N. or S. end of the needle attracted.	Deflections
Without the current	Unmarked	North	29°
Ditto	Marked	South	31°
Ditto	Ditto sustaining mag- net attached, }	Ditto	65°
With the current tending to neutralize the magnet . . . }	Ditto	Ditto	59°
Current and sustain- ing magnet removed }	Marked	Ditto	26°
	Unmarked	North	24°

124. I next place the cylindrical magnet under the influence of two sustaining magnetic bars, each 17 inches long; submitting it, at the same time, to the action of an electric cur-

rent, tending to neutralize it. The arrangement is represented by fig. 11, and the results were as follows :—

FOURTH SERIES OF EXPERIMENTS.

	Mean deflection of both poles of the Needle.
Before the magnet was subjected to the action of the current	30°
After the magnet had been subjected to a cur- rent tending to neutralize it	31°

125. When under the sustaining force of two magnets, we find that the electric current makes no impression on the small magnet on which it operated. The trifling power which the magnet gained during the experiment, was obviously due to the influence of the bars between which it was placed. The additional power given to the intervening magnet, by this means is, however, but very small, never amounting to more than 2° of deflection, as I have ascertained by several experiments, by permitting the cylindrical magnet to remain between the poles of the two large ones, as in fig. 11, for two minutes in each experiment ; which is a much longer time than it remained under the same influence after the removal of the electric current in the preceding experiments. Hence, since a sustaining magnetic force may be employed to any required extent, the obvious inference is this. *No electric current, however powerful, is capable of impairing the powers of a hard steel magnet, whilst the latter is under the protecting influence of a proper purely magnetic force.*

126. Having ascertained that the sustaining magnetic force does not operate as an exciting power (125), I was led to suppose that the power of the *protected* magnet is sustained by the mutual attractions of its own *disposable* forces (105, note) and those of the sustaining magnets: the north and south polar forces engaging with each other too intimately to be disunited by the assailing electro-magnetism in the helix. This view of the nature of the action led me to try soft iron as a means of sustaining the power of the magnet, whilst the latter was subjected to the action of an electric current, considering that a portion of the disposable force of the magnet would be employed by the iron, and thus be protected from the assailing electro-magnetic force ; but it was found by the experiments about to be described, that soft iron affords no protection whatever to the magnet when assailed by a converse electro-magnetic force: but on the contrary, the iron facilitates the subduction of the original powers of the steel magnet.

127. The experiments were made by placing the cylindrical magnet in the helix, and ascertaining its deflecting power on the needle at the original distance of 12 inches. Then placing in contact with its remote pole a cylindrical bar of soft iron, 6 inches long and about an inch in diameter. An additional deflecting force is thus given to the magnet, which deflection is also noted down. Another bar of soft iron, $3\frac{1}{2}$ inches long, and about the same thickness as the former, was next placed in contact with that pole of the magnet nearest to the needle, and the new deflection thus given to the needle also noted down. This done, the electric current from a new battery was transmitted through the helix, whose magnetic powers were opposed to the powers of the enclosed magnet. The following table shows the results:—

FIFTH SERIES OF EXPERIMENTS.

Deflections with the magnet in the helix, with and without the soft iron and electric current from a new battery.

With or without the soft iron and electric current.	Marked or unmarked end nearest to the needle.	N. or S. end of needle attracted	Deflections.	
Without current or iron	Marked.	South	30°	1
With the larger piece of iron . . .	ditto	ditto	42°	2
With both pieces of iron	ditto	ditto	65°	3
Do. with a converse electric current	ditto	North	40° then 19°	4
Current cut off, but iron remaining	ditto	South	25°	5
Magnet alone	ditto	ditto	10°	6

128. The principal circumstances to be noticed, in these experiments, are the singular changes of polarity by the soft iron, and the final subduction of a great portion of the force of the magnet. By deflection 4 we see a transposition of polarity by the action of the current. The new deflection thus given to the needle at first rose to 40°, but gradually sank down to 19°, where it remained permanent for some time. This reduction of the deflection was, of course, dependent on a reduction of polar energy in the nearest piece of iron: and as the polarity of the iron depended on the polar condition of the magnet, we learn that the transient transposition of its polarity is accomplished to the greatest extent, immediately after the current has got into full play, and that it gradually subsides for about one minute afterwards, at which time it has arrived at its minimum. These versatilities in the polar action

of the magnet are observable in all cases when it is subjected to a converse electro-magnetic action, whether there be any iron attached to its poles or not, though without iron they are not so great as when that metal is present. They are exceedingly curious, and are involved in a theoretical principle, which it is not necessary to enter into at present. By comparing deflections 1 and 6 we find that the magnet has lost a considerable portion of its power, which portion is greater by 6° or 8° than that usually lost when no iron is present, all other circumstances being the same; which shows that the attachment of the iron to its poles facilitates the subduction of the original powers of the magnet. See also the first and second series of experiments.

129. I had next recourse to the reverse process of that which was pursued in the last experiment. I placed the soft iron cylinder in the helix, and attached one pole of the cylindrical steel magnet to that extremity of it which was nearest to the needle; and whilst thus arranged, an electric current was transmitted through the helix. The distance between the pivot of the needle and nearest pole of the magnet was 12 inches. The following results were obtained:—

SIXTH SERIES OF EXPERIMENTS.

	Deflections.
Magnet alone, prior to being placed in the arrangement	38°
Magnet attached to the iron bar, the latter being under the influence of the current	45°
Magnet alone, after the iron and current were removed	38°

The magnetism of the soft iron left no additional permanent power on the steel magnet.

130. Having ascertained that an electric current is capable of subduing a considerable portion of the original power of an unprotected steel magnet (119, 120), it became an enquiry of some interest to ascertain whether or not the same current, with the magnet reversed in the helix, was capable of restoring the power which it had previously subdued. For this purpose, the cylindrical steel magnet was retouched; and after its deflecting power, at the distance of eight inches, had been ascertained, it was subjected to the action of an electric current from a perfectly new battery, whose copper exposed about a square foot of surface, with a proportionate rolled zinc cylinder inside. The battery was made' exceedingly active by a solution of nitro-sulphuric acid. The following table shows the results.

SEVENTH SERIES OF EXPERIMENTS.

Magnet alone, previous to its exposure to the current	39°
Ditto after being exposed to a <i>converse</i> current	21°
Ditto after being exposed to a <i>direct</i> current	25°
Ditto after a second exposure to ditto	26°
Ditto after several other exposures to ditto	26°

131. From this series of experiments, we learn that the active electric current here employed was incapable of restoring $\frac{1}{2}$ of that portion of the deflecting force, of a newly magnetized hard steel bar, which it was previously enabled to subdue, although as powerful during the one process as during the other. This exceedingly curious fact I have found in the results of several other experiments, and with batteries of different powers. But the same law does not hold good, unless the magnet has been magnetized to a high degree previously to its being subjected to the electric currents; nor, perhaps, will it be found *generally* exact, even under these circumstances, although I have not met with any results in direct contradiction to it. And although the ratio of the *subdued* and *restored* force may vary, I have cause to believe that in no case will the restored force be more than one-half of that which had been subdued by the same current, when the magnet employed is hard cast steel, and not below the dimensions of that which I have described (115): and the voltaic plates of proportional magnitude.

132. Another interesting fact presented itself by neutralizing the cylindrical steel bar, and afterwards magnetizing it by the electro-magnetic action in the helix, whilst the latter was transmitting a copious and active current from the battery last described (129), furnished with a new zinc. The deflecting power which the steel acquires, by this process, is about one-half of that which it exhibits by means of ordinary magnetic excitation. I have doubled and trebled the coil in the helix, but in no case has the magnetic power of the steel increased above that I have just mentioned. The facts developed by these experiments, are partly attributable to the magnetic force receiving different forms of distribution by the magnetic and electric processes of excitement; though principally from an absolute incapacity in the latter of bringing forth those intense magnetic forces which hard steel is susceptible of displaying. There seems, indeed, to be a vigorous tension in the magnetism of hard steel, which that of electric currents cannot compete with in vanquishing those formidable resisting forces presented by hard ferruginous bodies, whilst undergoing the magnetizing process. Even the magnetism of soft

iron, when brought into play by electric currents, though much more abundant in quantity, is of far lower tension than that of hard steel. This curious fact may be shown by experiments with two horse-shoe magnets; one of which shall be soft iron, brought into play by electric currents, and the other a permanent one of hard steel. When the cross pieces of both magnets are of soft iron, the iron magnet will have the greatest lifting power; but when both cross pieces are of hard steel, the steel magnet will have the greatest: and this is the case even when the power of the iron magnet (with soft iron cross pieces) exceeds the other to a considerable extent.

133. There is a remarkable phenomenon observed whilst magnetizing hard steel by electric currents. The deflecting power of the steel is much greater whilst under the dominion of the current than after the latter is cut off. Now, as the helix alone exhibits no action on the needle (118, 119), the experiment shows that there is a temporary disposable force excited even in hard steel, which that metal does not exhibit when the exciting cause is removed. This fact probably arises from a new distribution, rather than from an absolute loss of the magnetism first excited by the current.

134. Having ascertained that the existence of electric currents is nowhere to be found in permanent steel magnets, (114) and also demonstrated the inadequacy of electric excitement to the production of that extent of magnetic energy in hard steel, which is susceptible of development by the ordinary process of magnetization (131), it may now be interesting to inquire how far the doctrine of *systems* of electric currents is susceptible of application in explaining the phenomena exhibited by permanent steel magnets.

135. Let N and N', fig. 12. represent transverse sections of two cylindrical systems of electric currents, both of which are flowing in the same direction, as represented by the arrows: and let these cylinders be prolonged parallel to each other to any required distance behind the paper. Now, because of the electric currents on the adjacent sides of these cylinders running in opposite directions, in every pair of parallel sections, similar to those represented on the paper, those cylinders will exhibit a repulsion for each other throughout their whole length, or from end to end, according to the principles of electro-magnetism. Let, now, the remote extremity of the cylinder N' be turned towards the spectator, permitting the cylinder N to remain unmolested. Under these circumstances, the *same* extremities N, and N', of the two cylinders whose adjacent currents, in the former case, flowed in *opposite directions*, will now flow in the *same direction*, as may be

understood by looking at fig. 13: and consequently those extremities will attract each other. Again, let the arrows in fig. 14 represent the directions of two cylindrical systems of electric currents placed at right angles to each other, as C and C'. The adjacent portions of these currents flow in the same direction, and consequently will *attract* each other. Now place the electro-magnetic system C' in either of the positions represented by fig. 15, and it is seen that the adjacent currents in C and C' now flow in opposite directions, and will consequently *repel* each other.

136. From the above illustrations we learn that the extremities of two systems of electric currents will either attract or repel each other, according to the positions in which they are placed, and that they do not exhibit any specific polarity in the manner of ferruginous magnets, whose attractions and repulsions have no dependence whatever upon the positions in which their extremities are placed with respect to each other, but are invariably referrible to their specific polar character. There is, indeed, a striking distinction in the distribution of the magnetic force of steel bars, and that exhibited by electric conducting wires, whether the latter be in a simple strand, or coiled into any particular fashion. A conducting wire formed into a hollow helix displays but very little polarity exteriorly, in the direction of its axis (118, 119.), because of the inner and outer sides of the coil exerting their magnetic forces in opposite directions: but with hollow steel magnets, the polar forces of each individual extremity conspire with each other, and operate in concert upon vicinal ferruginous matter, whether previously polarized or otherwise; and in precisely the same manner as such matter is operated on by *solid* magnets. Hence it is, that a polarized needle, or small bar, freely suspended, with its centre in the equatorial plane of a hollow steel magnet, whether *inside* or *outside* of the tube, will invariably assume one and the *same* direction: whereas a similarly suspended needle, with reference to, and under the influence of, a hollow system of electric currents, would assume *one* direction when *within*, and the opposite direction when *without*, the system: and as this peculiarity of magnetic arrangement would attend every system of electric currents that can possibly be formed, it is just to infer that the distribution of force displayed by steel magnets, or by loadstone, cannot be imitated by any system of electric currents whatever: and *vice versâ*, the exquisitely uniform arrangements of enveloping magnetic action, so beautifully displayed around electric currents, appear to be totally inimitable by any known forms of ferruginous magnetic bodies.

137. It would be an almost endless task to examine every fact that might be brought to bear, directly, or indirectly, on the subject of this investigation. I have not dwelt on electro-magnetism to the extent I would have done, had my theoretical views on that department of electricity not been already before the public, although I have cited those electro-magnetic phenomena which appear to be the most important in the present discussion. In other departments of electricity I have enumerated such facts as have appeared necessary to collate with purely magnetic phenomena; and having discussed them individually as I have proceeded, a retrospection would be needless in this place. The inference to be drawn from the investigation of the facts alone, appears to me to admit neither of doubt nor equivocation; and may be thus briefly stated: *There are no facts on record which demonstrate an identity in electricity and magnetism; but, on the contrary, there are many phenomena which justify the idea of their being perfectly distinct powers of nature.*

IV. *On the use of Electro-magnets made of iron wire for the Electro-magnetic engine. By J. P. JOULE, Esq. Communicated in a letter to the Editor.*

Salford, March 27, 1839.

Dear Sir,

In my last letter I gave you an account of some experiments which were intended to prove that electro-magnets made of iron wire are the most suitable for the electro-magnetic engine. In those experiments round wire was used,—and it was my opinion, that the wire magnets were put in a disadvantageous position, in consequence of the interstices between the wires. I have since confirmed my views on this subject by the following experiment:

I constructed two magnets. The first consisted of 16 pieces of square iron wire, each $\frac{1}{4}$ inch thick, and 7 inches long, bound very tightly together so as to form a solid mass, whose transverse section was $\frac{1}{4}$ inch square; it was then enveloped by a ribbon of cotton and wound with 16 feet of covered copper wire, of $\frac{1}{8}$ inch diameter. The second was made of solid iron, and was in every other respect precisely like the first. These magnets were fitted to the apparatus used in my former experiments, and care was taken to make the friction of the pivots equal in each. The mean of several experiments gave 162 revolutions per minute for the first, and 130 for the second magnet.

In the further prosecution of my enquiries, I took 6 pieces of round iron of different diameters and lengths, and 1 piece of hollow round iron, $\frac{1}{8}$ of an inch thick; these were bent into the U form, so that the shortest distance between the poles of each, was half an inch; each was then wound (with the usual precautions to ensure insulation), with 10 feet of covered copper wire, $\frac{1}{16}$ inch in diameter. The lengths and diameters are given in the table. No. 1 is the hollow magnet. The attraction was ascertained by suspending a straight steel magnet, $1\frac{1}{2}$ inch in length, horizontally to the beam of a balance, and bringing the several magnets directly underneath at the distance of half an inch, which was preserved by the interposition of a piece of wood. Care was taken that the battery remained constant during the experiment.

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.	No. 7.
Length in inches.	$\frac{1}{2}$ 6	$\frac{1}{2}$ 5 $\frac{1}{2}$	$\frac{1}{2}$ 2 $\frac{1}{2}$	$\frac{1}{2}$ 5 $\frac{1}{2}$	$\frac{1}{2}$ 2 $\frac{1}{2}$	$\frac{1}{2}$ 5 $\frac{1}{2}$	$\frac{1}{2}$ 2 $\frac{1}{2}$
Diameter in inches.	$\frac{1}{2}$ $\frac{1}{8}$	$\frac{1}{2}$ $\frac{1}{8}$	$\frac{1}{2}$ $\frac{1}{8}$	$\frac{1}{2}$ $\frac{3}{8}$	$\frac{1}{2}$ $\frac{3}{8}$	$\frac{1}{2}$ $\frac{1}{4}$	$\frac{1}{2}$ $\frac{1}{4}$
Weight lifted in ounces.	$\frac{1}{2}$ 36	$\frac{1}{2}$ 52	$\frac{1}{2}$ 92	$\frac{1}{2}$ 36	$\frac{1}{2}$ 52	$\frac{1}{2}$ 20	$\frac{1}{2}$ 28
Attraction in grains.	$\frac{1}{2}$ 7.5	$\frac{1}{2}$ 6.3	$\frac{1}{2}$ 5.1	$\frac{1}{2}$ 5.0	$\frac{1}{2}$ 4.1	$\frac{1}{2}$ 4.8	$\frac{1}{2}$ 3.6

A steel magnet of such dimensions as enabled me to compare it fairly with the rest, excited in the same circumstances an attractive power equal to 23 grains, while at the same time its lifting power was only 60 oz.

These results will not appear surprising if we consider, first, the resistance which iron presents to the induction of magnetism: and, secondly, how very much the power of iron to conduct magnetism is exalted solely by the completion of the ferruginous circuit. In order, however, to explain why the long electro-magnets have a *greater* attracting power, and lift *less* weight, than the short magnets of the same diameter, it will be necessary to observe that it was impossible to wrap the whole 10 feet of wire on the smaller magnets, without disposing it in two or even three layers (according to the size of the magnets): this is a great disadvantage, and one might anticipate in consequence that the power of the long magnets should be greater than that of the short for lifting as well as for attraction, contrary to the results in the table; this, however, may be explained, if we admit that the comparative resistance of the iron of the electro-magnet increases to a very great amount, when its magnetism is so greatly excited by the contact of the armature.

Nothing can be more striking than the difference of the ratios of lifting to attractive power, in different magnets; whilst the steel magnet attracts with the force of 23 grains and lifts 60 oz., No. 3 attracts 5.1 grains and lifts 92 oz.

Here are some very general directions for making electro-magnets for lifting. 1st. The magnet, if of considerable bulk, should be compound; and the iron used, of good quality and well annealed. 2d. The bulk of the iron should bear a much greater ratio to its length than is generally the case. 3d. The poles should be ground quite true, and fit flatly and accurately to the armature. 4th. The armature should be equal in thickness to the iron in the magnet.

I shall now proceed to consider with greater care, what form of electro-magnet is best for distant attraction, as that is the only force of any use in the electro-magnetic engine. Here two things must be considered—the length of the iron, and its sectional area.

Now with regard to the length of the iron, I have found that its increase is always accompanied with disadvantage, unless the wire is (by using a shorter length) forced to too great a distance from the iron. In making magnets for the engine it will be proper to use a length less than that which gives the maximum of attraction, on several accounts.

The next thing to be considered is the sectional area. You have shown,* that on placing a hollow and solid cylinder of iron successively within the same electro-magnetic coil, the hollow piece exerted the greatest influence on the needle. I wished to ascertain whether a hollow magnet might be represented by a solid one whose sectional area and circumference is the same, and whose thickness is twice as great as that of the hollow magnet. Fig. 12 and 13 Plate I. will show more clearly what I mean: they represent sections of a hollow and a rectangular magnet, and it will be seen that if either of them is divided at the dotted lines, the separate pieces when put properly together, will make up the other. Two electro-magnets were constructed, each 7 inches long, and covered with 22 feet of covered copper wire $\frac{1}{16}$ inch in diameter; the sections were precisely similar, but double the size of those in the figures. Here is their actual attraction, at half an inch distance, for the proper pole of a straight steel magnet.

	Hollow magnets.	Solid magnets.
Attraction in grains	1.9	1.7
With a more powerful battery	4.5	4.0

* See the very interesting researches at page 470, Vol. I.

It is evident from this that the hollow magnet has the greatest attractive force, but I do not think that the difference is so great as to counterbalance the many advantages which the solid magnet would give, if used in the engine. I shall therefore first relate a rather important experiment; and secondly make an attempt to determine the sectional area of solid iron most proper for different powers of battery.

I made five straight magnets of square iron wire $\frac{1}{16}$ inch thick: each was 7 inches long and wound with 22 feet of covered copper wire $\frac{1}{8}$ inch in diameter. No. 1 consisted of 9; No. 2 of 16; No. 3 of 25; No. 4 of 36; and No. 5 of 49 wires; arranged in the form of a prism with square base and sections. Five other magnets were made of solid iron, but in every other respect exactly similar to the first. Here are the attracting powers (at half an inch) for a straight steel magnet, with three different galvanic forces.

		No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
1st Ex.	Attraction of iron magnet in grains.	1.5	1.9	1.6	2.1	2.0
	Ditto of wire magnet	2.1	2.1	1.7	2.0	1.9
2d Ex.	Iron magnet. . . .	2.0	2.5	2.35	2.45	2.2
	Wire ditto.	2.6	2.8	2.1	2.2	2.05
3d Ex.	Iron magnet. . . .	2.7	3.6	3.4	3.2	3.1
	Wire ditto.	3.3	3.8	3.0	2.9	2.65

The wire used in these magnets was taken at the same degree of temper, as that in which it came from the makers: it was in consequence not so well annealed as the iron with which it was compared. On this account the numbers opposite to the wire magnets are less than they would otherwise be; still, however, the results in the table seem anomalous. First, it will be remarked that while the wire magnets are more powerful in the first numbers, they are less powerful in the last numbers, than the iron magnets. I cannot account for this unless by supposing, according to the hypothesis of Dr. Page, that the wires of which the magnets are composed repel one another's magnetism in such a manner as to tend to neutralize the general force of the electro-magnet, and that this neutralizing effect increases with the number of wires used. But the deficiency of No. 3 magnets in ex. 1. is most remarkable, and particularly as by increasing the power of the battery the deficiency is reduced, and that at the same time the wire magnet becomes less, though at first it was more, powerful than the iron magnet, compared with it.

In my next, I shall attempt to determine what sectional area is best for different electric forces. In the mean time,

I remain, dear sir,
Yours most respectfully,
J. P. JOULE.

V. *Note on illuminating Gas, and particularly on the formation of Gas from water by means of M. Selliques's apparatus. By M. GROUVELLE. (Extract).**

M. Selliques never said that water by passing over coke heated to redness, was transformed into *carbonated hydrogen*. It is well known that it produces a mixture of oxide, carbon, and hydrogen, almost entirely pure.

But M. Selliques charges this *hydrogen* with carbon by causing it to traverse a *red-hot* cylinder, when it meets with highly carbureted oils. It is a chemical combination and not a mixture which then takes place, as is proved by the analysis of the gas formed from water, by M. Peligot, Repetiteur à l'Ecole Polytechnique; viz.—

Carbonated Hydrogen	57	} 100
Oxide of Carbon	28	
Free Hydrogen	15	

Hence the theoretical question of lighting is this: What process gives the most light with one kilog. of oil or any resinous substance, resin, schist, or coal tar, &c.? One kilog. of oil of schist or resin furnished, by Selliques's apparatus, 70 *cubic feet* English of burning gas, of which it requires 3 feet to supply a burner equal to 10 wax candles for one hour; which gives 23 hours' light.

But at Belleville, Antwerp, Frankfort, and wherever gas is made on a large scale from *oil of resin* and a portion of pure resin, the mean product is from 15 to 17 *feet* per kilog. of oil, but in three or four days the product falls to 12 and 15 *feet*. The insulated attempts, with new retorts, may give as much as 24 or 25 feet: and M. Tailleberg has pronounced this production of 25 feet as a great discovery. Let us take this number: this gas burns $2\frac{1}{2}$ feet per hour to give the light of 10 wax candles; this is (although nearly double the mean) the report furnished by the lighting of the City of Antwerp, in October, 1837, with gas from resin at 12 feet to the kilog., and in October, 1838, with gas from water. We will only reckon upon $2\frac{1}{2}$ feet; hence, 1 kilog. of oil gives at the maximum 11 hours' light, and admitting even $3\frac{1}{2}$ feet to the kilog.,

* From the Comptes Rendus, &c. No. 23, 1838. Translated by Mr. J. H. Lang.

as has been stated in a journal (and this quantity cannot perhaps be obtained without the addition of water), it would then be only 15 hours while we have obtained 23 from the gas made from water.

But the production of gas from water does not stop at 70 feet per kilog. By increasing the proportion of water to oil in the apparatus, we weaken the density of the gas, which approaches to, and even descends lower than, the density of the coal gas. In some experiments made on more than 1500 feet, observed for several consecutive hours and proved by a tedious process, I carried the production to 222 feet of burning gas with 1 kilog. of fish oil (the oil of schist, which I had not at this time, gives in Selliques's apparatus the same results as fish oil).

This gas at 222 feet only burnt $6\frac{1}{2}$ feet to give the light of 10 wax candles; it was scarcely $\frac{1}{3}$ weaker than coal gas. Gas produced at 110 feet to the kilog. of oil of schist gave me a consumption of 4 feet 20 for the same burner. Thus at about 160 feet to the kilog. of oil, the gas from water is equal in power to coal gas, and burns 5 feet an hour. Hence, 1 kilog. of oil gives 40 *hours' light*. It is easy to calculate what this light costs with oil of schist, which, in the places of its production, is not more than 5 francs per 100 kilog., with a combustible expenditure which decreases as the proportion of the gas produced and the size of the apparatus increase. With the gas from resin, on the contrary, the decomposition of the oils operating at the melted surface, small retorts are the most advantageous, and at the same time, the volume of the gas produced (not its mass) is increased only in decarbureting, by a higher temperature, a part of the richest carburets of hydrogen.

The indefinite increase of light obtained with the gas from water, in proportion as it is produced more feeble, tends to prove that the presence of the oxide of carbon increases the illuminating power of this gas, doubtless increasing the quantity of heat developed during the combustion.

We learn from two reports made to the Antwerp Society for lighting with gas, that from the 1st of June, the City of Antwerp was lighted with the greatest success by the gas from water; that three furnaces are in active employ and produce from 24 to 25 thousand cubic feet of gas per day; notwithstanding the useless expense with which the Society is charged, the gas at 70 feet to the kilog., costs, workmanship and keeping up the furnaces included, less than 5 francs per 100 feet with oil of schist at 15 francs per 100 kilog.

The superior quality of the gas from water to that from coal, on account of its total absence from sulphur and ammo-

nia, need not be discussed; and on the other hand, the cost of this gas is no longer in doubt: at Antwerp, at Belleville even, it has been proved in every way.

At Antwerp, the burner, equivalent to 10 wax candles, consumes at the rate of 3 feet, 1^c.35 per hour, and costs 4 francs 50 cents per 100 feet.

At Paris, the coal gas (which is not obtained for nothing as has been pretended) costs, as the companies know, from 4 to 5 francs per 100 feet, including workmanship, washing, and keeping up the apparatus; at Mons, even the director allows 3 francs. At London, it costs 2s. 6d. or 3 francs 12 cents.

But it would not be difficult for us, with oil of schist at 6 or 8 francs, to produce in London, or even in Belgium, gas from water, at the rate of 160 feet to the kilog., equal in power and superior in quality to the coal gas, at less than 3 francs per 100 cubit feet English.

I shall add a few words on the employment of asphaltum pipes.

The question is not what pressure asphaltum pipes will sustain, that of the gasometers being always infinitely small, *but how they will support the chemical and slow action of the gas itself, on the asphaltum.* After some experiments, unfortunately too short, several thousand stone pipes were placed in Louvain, with well baked *asphaltum* joints, and at the end of four or five months most of these joints were eaten and pierced by the gas, which no doubt attacked them by means of the small quantity of essential oil, which it carries in its vapours. It was necessary to replace all the resinous joints by others of clay, covered with Roman cement, and up to the present time the results appear good.

VI. *Description of a Voltaic Battery.* By S. E. HOSKINS,
M. D. *Communicated in a letter to the Editor.*

Sir,

The plate voltaic battery, although nearly superseded by the more modern cylindrical arrangements, is still of sufficient value to sanction an endeavour towards improving its construction.

If the following description of a method I have devised for its simplification be worthy of a place in your excellent Annals, I shall be obliged by its insertion.

The various inconveniences arising from the usual methods of connecting galvanic plates, and the difficulty of cleansing the zincs when soldered to the coppers, induced me some time

ago to seek a more simple mode of effecting junction. The result is a method whereby solder, mercury cups, and binding screws are dispensed with; and perfect connexion between a dozen or eighteen pair of plates effected by the mere adjustment of a couple of thumb screws.

Fig. 14, Plate I., represents a wooden frame with fine transverse saw cuts, half an inch deep, and one fourth or one sixth of an inch apart. This frame is intended to fit over a trough without partitions. Each of its projecting extremities is perforated for the passage of a brass bolt, having a head at one end and a deep thread for the reception of a thumb screw at the other.

The plates which are dropped into the transverse saw cuts of the frame, alternately interlacing, according to Messrs. De la Rue and Young's plan, are cut out of copper and thin sheet zinc.*

The plates being dropped into the frame, the long ears are to be bent over its convex sides, so that a zinc shall be in contact with a copper plate. This being done a strip of wood is placed over the bent ears: over this a strip of brass and the whole bound together by the bolts and thumb screw, until perfect contact is secured.

This arrangement will be better understood by reference to fig. 14, of the accompanying sketch, which gives a transverse view of the apparatus.

a, a, a, a, are the plano-convex sides of the frame. *b, b, b, b*, the alternating plates of zinc and copper. *c, c, c, c*, thin strips of white deal varnished, and barely long enough to cover the series of overlapping ears. *d, d, d, d*, strips of thick sheet brass, as long as the sides of the frame, and perforated at each extremity. *E, E*, bolts and thumb screws, which are represented as having tightened the binders on one side, and in readiness to do so on the other.

The dotted lines, in fig. 15, are intended for pieces of varnished cord permanently fixed on the copper plates: a simple but effectual method of keeping the plates asunder.

The battery may be used with dilute acid or with the acidulated solution of sulphate of copper recommended by M. De la Rue. With the latter, good decomposing action will continue for upwards of an hour; at the end of which time three inches of fine platinum wire can be kept in a state of in-

* The thinner the better as it can be readily cut and bent, does not require wide saw cuts, which would weaken the frame;—lasts quite long enough for one durable operation, and admits of fresh plates being used each time.

candescence for half an hour or more. In short, the power of the battery is quite equal to that of other plate arrangements possessing advantages peculiar to itself. These advantages are convenience in an extended sense of the word, and economy both of time and money. Eighteen or twenty pair of plates may be sundered and put together again in a few minutes; the zincs therefore may be easily washed, amalgamated, or replaced, with great facility and without the use of the soldering tool, the waste of mercury, or the annoyance arising from a series of binding screws. A stock fashioned by the manipulator may always be kept at hand with no more expense than that of the material; and he may extend his series with ease to any extent.

The only parts which cannot in general be made by the amateur are the trough, the screws, and the frame. The latter requires some degree of nicety in its construction,—none however which a common carpenter, properly directed, may not attain.

A small battery, such as I have described, has been deposited at the Polytechnic Institution, ever since the month of October. Mr. Bachhoffner, to whose kindness on all occasions I am much indebted, has performed a series of carefully conducted experiments with it, and allows me to state in his name that my battery is much more powerful than others of the same order, owing to the approximation of the plates, and that it is much more convenient and manageable than any.

I remain, Sir,

Guernsey, May, 1839.

Your obedient servant,
S. E. HOSKINS, M. D.

VII. *On a New Magnetic Electrical Machine. (Magnet-electromotor.)* By DR. NEEFF, of Frankfort.*

Exhibited at the Friburg Meeting of Philosophers, in Sept., 1838.

Since the time I made known the peculiarities of my electrical wheel, or mill, to the scientific meeting at Bonn, (and afterwards in Poggendorff's *Annals* for November, 1835,) the remarkable effects of electrical discharges, repeated in rapid succession, have become studied with much attention: and it was soon discovered that, for the production of such a quick succession of electrical light, magnetic electricity is peculiarly and excellently adapted. For this purpose magnetic electricity has been employed as it is excited by the machine first invented

* Translated from the German by I.

by Pixii, then by Saxton's and Clarke's improvements; each of which has an armature of soft iron, surrounded by spirals of copper wire, which rotate in front of the poles of a steel magnet, by which arrangement a machine is brought into good operation; and the progressive improvements which the sagacious and ingenious Ettingshausen has given to this machine, are so excellently contrived, that there appears to be left but little more to be done to accomplish its perfection. In the meantime, however, I have been inclined to believe that some other way must yet be pursued to arrive at the principal object in view: which would be to replace the steel magnet by an electro-magnet of soft iron. The first effect which I obtained by this substitution, was far short of that which I had been led to expect, a circumstance that may be imputed to the deficiency which attended the first construction of the instrument. The essential corrections being, however, attained, the magnetic electrical operation can now be brought to the wished for vigour; the apparatus is easy and convenient to manage, durable in its action, of small dimensions, its price trifling, and is well adapted for a variety of purposes which are met with by the Surgeon, the Physiologist, and the Physician. These results have appeared to me so highly gratifying that I am led to believe the instrument is still susceptible of much farther improvement.

With respect to the voltaic battery, I have relied upon those liquids which I have hitherto been in the custom of using for the excitation of voltaic troughs, but have returned to the oldest construction of voltaic apparatus, viz, piles of zinc and copper, with intervening discs of moistened paper. The zinc, however, I amalgamate. When the paper discs have become saturated in a solution of sulphuric acid, (consisting of about one acid and ten water) I form the pile, and place it in a screw-press. The performance, of this construction of a battery, is extremely uniform and durable. Experiments may be carried on daily for a considerable time and still the battery will continue active. Even for 12 or 16 days, experiments may be carried on before it is necessary to take the pile to pieces and introduce fresh discs of paper. Besides, the metals become so little corroded that they seldom want any other cleaning than merely wiping away the moisture from about their edges, a circumstance always to be attended to, otherwise a perceptible portion of the force is lost. With this pile, it is pleasing to find that we are not annoyed by the troublesome, indeed dangerous, liberation of gas, which always attends the apparatus when unamalgamized zinc is employed. The screw is also of very great advantage, by the slackening

or tightening of which, the effects of the pile can quickly be weakened or strengthened respectively, at pleasure. I use plates of greater dimensions and number, as is necessary for the maximum of effects. Though but little is to be gained, in this way, for the present purpose of the pile, yet it gives me a great advantage of diversifying the force in many ways. The action becomes exhausted in, perhaps, about 14 days. In the manner above described, I prepared 8 copper and zinc plates in four pairs, insulating them from each other by dry paper, and keeping them pretty close together by means of the screw-press, and combined, as occasion required, the similar or dissimilar metals, by means of small conducting wires and quicksilver cups. The eight moistened papers which were placed between the copper and zinc were 4 inches broad and $4\frac{1}{2}$ long, the plates being a little larger. The screw-press is, perhaps, 7 inches long and 6 inches broad, and serves as a basis for the support of the other parts of the apparatus.

The second essential part of the apparatus is the spiral. The principle of which is well known, as far as depends upon the length, thickness, and winding of the wire. The iron axle of the spiral, in every revolution, necessarily weakens the electric action, in consequence of a partial neutralization of the magnetic poles, by its close approach to them during its transits. As regards the function of the spiral, it is now known that, when it closes the circuit, the iron axle becomes magnetic; and, in opening the circuit, this magnetism, as well as that of the wire, immediately disappears; by means of which, the electric fluid in the spiral becomes impelled, and exhibited partly in a spark in the reverse order to the battery circuit, and is partly led off as a momentum current. The best method of a spiral is to have two wires wound close together. We can then employ it according to the various purposes for which it is wanted; we can combine the two wires in the *same* direction or in opposite directions; we can even, by the one, close and open the circuit, and by the other, lead away the magnetic electricity.

The third element of the magnet-electromotor is the mechanical, by which the closings and openings of the circuit through the spiral are performed. For this purpose, I first availed myself of the electric wheel, by which contrivance the shocks succeed one another with great velocity, and become well defined. When I had ascertained the powerful action of the apparatus, I was desirous of having it to excite itself, as in the electro-magnetic machine, without the inconvenience of turning the wheel. The ingenious construction which I employ for the purpose is due to Mr. J. P. Wagner.

It combines simplicity with efficacy, and is the result of a variety of contrivances which have now been carried on for two years: and was given to me by that gentleman. There are two parts inserted between the voltaic series and the spiral, which I call hammer and anvil. The hammer is a piece which is attached to one end of the spiral, (the other end being connected with one pole of the series) and the anvil is connected with the other pole of the series. If now, the hammer rests upon the anvil, the circuit will be closed, and the iron axle, becoming magnetic, attracts an iron plate which fastens to the hammer, and draws it away from its contact with the anvil. By these means the circuit will be opened, and the iron axle immediately loses its magnetism, which permits the hammer to fall down again to the anvil and again closes the circuit. This done the same motions begin anew, and are repeated as long as the pile retains its power. The hammer may be brought to any required distance from the anvil at pleasure, and mercury may be employed between them if thought necessary: and there is also a contrivance for elevating or depressing the anvil. These modifications of the apparatus permit the rapidity of the openings and closings to be varied in several ways.

The apparatus operates upon principles already known. The various combinations of the spiral wire serve the purpose of attaining various objects. If, for instance, we require a great *quantity* of the electric force, we must unite the ends of both spiral wires which are of the same name; by which means we shall have sparks and chemical decompositions at a maximum. On the other hand, if we desire a maximum of *intensity*, then we must unite the ends of the wires which are of *different* names, by which means the greatest effect is produced on inferior conducting bodies. The fiery sparks appear between the hammer and anvil. The decompositions and shocks are obtained by bringing the respective bodies into contact with one end of the spiral wire; also to the quicksilver shank to which the hammer is united, and to that pole of the series which touches the other end of the spiral. Of the experiments I shall mention only one; the deflagration of various metals, and particularly of quicksilver under water. The convulsions of this metal are shown by placing a drop of it under acidulated water and touching it with one end of the spiral wire; a rotation immediately commences in the water; and, by introducing a charcoal point, the phenomenon becomes striking and beautiful. The action on the human body is extremely powerful. When the spiral wire is only 400 feet long we find that lively shocks are given even when the poles

are touched with dry fingers, which, by a somewhat stronger pressure, become increased to an insufferable degree. By a moderately weak contact, one hears a gentle crackling noise, which appears to arise from a series of small sparks passing through the insulating epidermis. By moistening the finger in water, one gets only a tolerable superficial connexion, which continues but for a few seconds when the action is strong. The intensity is sufficiently great to transmit shocks through a series of many persons who are connected with moistened hands. A very interesting experiment is made by the employment of two polar plates, by means of which we obtain a current through a mass of water, and the human body, or even a hand immersed between these plates, is acted upon by the current. In this electrical bath, independently of any direct contact with the polar plates, the immersed body deprives the water of the greatest part of the electrical current, and, consequently, a lively sensation is experienced at every point of the immersed part. The importance of such electrical baths for medical purposes is very easily perceived, and ought to be strictly attended to by the faculty.

Finally, by augmenting the length and thickness of the spiral wire, the force becomes exalted, and answers for every purpose; and one can predict with certainty that it is capable of decomposing the alkalies. It is better adapted for this purpose when, instead of wire, a copper band, (a strip of sheet copper) in about twelve even spirals, is wound round the axle, having the inner and outer ends prepared with quick-silver cups for the purpose of varying the connexions. On this plan my Reometer (an instrument described in Gehler's Phil. Dictionary, new edition, vol. vi., sec. 3, p. 2494) is constructed and brought into action.

VIII. *Notice from DR. ROBERT HARE, Professor of Chemistry, &c., respecting the fusion of platina, also respecting a new Ether, and a series of gaseous compounds formed with the elements of water.**

I have by improvements in my process for fusing platina, succeeded in reducing twenty five ounces† of that metal to a state so liquid, that the containing cavity not being sufficiently capacious, about two ounces overflowed it, leaving a mass of twenty three ounces. I repeat that I see no difficulty in

* Communicated by the Author.

† Troy weight. The actual quantity fused was 12,250 gra.; the lump remaining weighed 10,937 grs.

extending the power of my apparatus to the fusion of much larger masses.

When nitric acid or sulphuric acid with a nitrate is employed to generate ether, there must be an excess of two atoms of oxygen for each atom of the hyponitrous acid which enters into combination. This excess involves not only the consumption of a large proportion of alcohol, but also gives rise to several acids and to some volatile and acrid liquids.

It occurred to me that for the production of pure hyponitrous ether a hyponitrite should be used. The result has fully realized my expectations.

By subjecting hyponitrite of potassa or soda to alcohol and diluted sulphuric acid, I have obtained a species of ether which differs from that usually known as nitrous or nitric ether in being sweeter to the taste, more bland to the smell, and more volatile. It boils below 65° of F., and produces by its spontaneous evaporation a temperature of $0-15^{\circ}$ F. On contact with the finger or tongue it hisses as water does with red hot iron. After being made to boil, if allowed to stand for some time at a temperature below its boiling point, ebullition may be renewed in it apparently at a temperature lower than that at which it had ceased. Possibly this apparent ebullition arises from the partial resolution of the liquid into an aeriform ethereal fluid, which escapes, both during the distillation of the liquid ether and after it has ceased, at a temperature below freezing. This aeriform product has been found partially condensible by pressure, into a yellow liquid, the vapor of which, when allowed to enter the mouth or nose, produced an impression like that of the liquid ether. I conjecture that it consists of nitric oxide, so united to a portion of the ether as to prevent the wonted reaction of this gas with atmospheric oxygen. Hence it does not produce red fumes on being mingled with air.

Towards the end of the ordinary process for the evolution of the sweet spirits of nitre, a volatile acrid liquid is created which affects the eyes and nose like mustard, or horse radish.

When the new ether as it first condenses is distilled from quick-lime, this earth becomes imbued with an essential oil which it yields to hydric ether. This oil may be afterwards isolated by the spontaneous evaporation of its solvent. It has a mixed odour, partly agreeable, partly unpleasant. From the affinity of its odor and that of common nitrous ether, I infer that it is one of the impurities which exist in that compound.

The new ether is obtained in the highest degree of purity, though in less quantity, by introducing the materials into a

strong well ground stoppered bottle, refrigerated by snow and salt. After some time the ether will form a supernatant stratum, which may be separated by decomposition. Any acid, having a stronger affinity for the alkaline base than the hyponitrous acid, will answer to generate this ether. Acetic acid not only extricates but appears to combine with it, forming apparently a hyponitro-acetic ether.

I observed some years ago that when olefiant gas is inflamed with an inadequate supply of oxygen, carbon is deposited, while the resulting gas occupies double the space of the mixture before explosion. Of this I conceive I have discovered the explanation. By a great number of experiments, performed with the aid of my barometer gauge Eudiometer, I have ascertained that if during the explosion of the gaseous elements of water any gaseous or volatile inflammable matter be present, instead of condensing there will be a permanent gas formed by the union of the nascent water with the inflammable matter. Thus two volumes of oxygen, with four of hydrogen, and one of olefiant gas, give six volumes of permanent gas, which burns and smells like light carburetted hydrogen. The same quantity of the pure hydrogen and oxygen with half a volume of hydric ether gives on the average the same residue. One volume of the new hyponitrous ether under like circumstances produced five volumes of gas.

An analogous product is obtained when the same aqueous elements are inflamed in the presence of an essential oil. With oil of turpentine a gas was obtained weighing per hundred cubic inches $16\frac{1}{8}$ grs., which is nearly the gravity of light carburetted hydrogen. The gas obtained from olefiant gas, or from ether, weighed on the average, per the same bulk $13\frac{1}{8}$ grs. The olefiant gas which I used weighed per hundred cubic inches only $30\frac{1}{8}$ grs. Of course if per se expanded into six volumes it could have weighed only one sixth of that weight, or little over five grains per hundred cubic inches. There can therefore be no doubt that the gas obtained by the means in question, is chiefly constituted of water, or of its elements in the same proportion H^2O .

With a volume of the new ether, six volumes of the mixture of hydrogen and oxygen give on the average about five residual volumes. The gas created in either of the modes above mentioned does not contain carbonic acid, and when generated from olefiant gas appears by analysis to yield the same quantity of carbon and hydrogen as that gas affords before expansion.

These facts point out a source of error in experiments, for analyzing gaseous mixtures by ignition with oxygen or hydrogen, in which the consequent condensation is appealed to as a

basis for an estimate. It appears that the resulting water may form new products with certain volatilizable substances which may be present.

IX. REVIEWS AND NOTICES OF NEW BOOKS.

A Course of Eight Lectures on Electricity, Galvanism, Magnetism, and Electro-magnetism. By HENRY M. NOAD, *Member of the London Electrical Society.* SCOTT, WEBSTER, and GEARY, 36, *Charter-House Square.*

Mr. Noad's Lectures, like some other recent publications purporting to treat on these subjects, are, principally, if not totally, compilations from other sources, to no one of which have they made even the slightest contribution. Mr. Noad has obviously read several authors on the subjects of his Lectures, and, of course, has selected those parts and parcels of their works which appeared most suitable to his purpose. It is our duty, however, in justice to its author, to state, that Mr. Noad has drawn, to a rather unusual extent, on Sir David Brewster's "Treatise on Magnetism," many parcels of which, already in a suitable dress for the "Lectures," of course, required only the mere process of transplantation from one work to the other. We do not, however, find fault with Mr. Noad for thus availing himself of the matter in its original elegant form, because it is well chosen and well adapted to the purpose; and, as he has not attempted to conceal his authorities, but, in general, has been very liberal in acknowledging them, his "Lectures" claim our best wishes for their success, sincerely hoping that they may produce a better effect than that of *beguiling an idle hour*, which the preface informs us is the only "object of the author" for offering them to public notice.

"Magnetical Investigations." By the Rev. WM. SCORESBY, B. D., *Fellow of the Royal Societies of London and Edinburgh; Corresponding Member of the Institute of France, &c., &c.* LONGMAN, ORME, BROWN, GREEN, and LONGMANS, *Paternoster Row.*

We have, in this work, a series of magnetical observations and facts of exceedingly great interest, whether they be viewed in a theoretical point of view, or of practical applicability. The labours of Mr. Scoresby, as an experimental philosopher, have long been sufficiently known to the scientific world to

establish his reputation as an indefatigable and exact investigator; and we have great pleasure in stating that the originality of many facts which we have observed in this neat little volume, do equal credit to the author with any of the preceding results of his valuable investigations.

Mr. Scoresby prefaces his interesting *Investigations with the following series of "Introductory Observations,"* so admirably appropriated to the scientific importance, and theological dignity, of his subject.

"It had long been conjectured that a most intimate connexion, if not identity of nature, might probably exist among some of the more subtle and mysterious agents, or principles of different denominations, and, apparently, of different characteristics, which universally pervade the region in and about this earth.

"Modern discoveries in electro-magnetism, with the cognate relations which these have developed as existing in other principles of natural bodies, have gone far to verify these anticipations; and at the same time to yield so much additional knowledge of the constitution of the physical system of our planet, as to give a new, an interesting, and a prominent importance to, electrical and magnetical science.

"To the time of Dr. Gilbert, of Colchester, magnetism was only known as a mysterious virtue, existing in and peculiar to, the loadstone, or ferruginous substances which had been touched by this extraordinary mineral, from which certain qualities of attraction and direction were derived. But this eminent individual discovered, as he has left on record in his '*Physiologia Nova, seu Tractatus de Magnete et Corporibus Magnetis*;' published in the year 1600, that the phenomenon of the meridional adjustment of the magnetic needle was not owing to any mystical virtue, exercised out of the course of natural principles, but a mere result of the directive action of the earth; which he truly considered as the controlling agent, by reason of its being in its matter and constitution magnetic.

"So long as magnetism was known only as a separate or simple principle, the philosophic ideas of Dr. Gilbert were never materially advanced; but, on the discoveries of Professor Oersted, whereby the long-suspected connexion betwixt electricity and magnetism was established, an amazing enlargement was at once yielded to magnetical knowledge, and a corresponding impulse given to magnetic research.

"The effect has been to give a science, formerly considered as comparatively of an inferior class, a grandeur of consideration; placing it at once amongst those mighty principles which

infinite wisdom has appointed, and infinite power ordained as essential elements or agencies in the physical constitution of the world. For inasmuch as the inseparable connexion has been established betwixt electricity and magnetism, these, under various forms of development, reciprocally developing each other, it necessarily follows, that, to whatever extent in creation electricity operates, to the same extent the magnetic principle must reach. And inasmuch as heat, light, and chemical action, are each, more or less, developers of one form or another of the electro-magnetic principle, the analogies of science would lead us to infer, that magnetism is co-extensive with these other agencies, throughout their range of operation. Hence, there is little doubt but the principle, which, a few years ago, was known only as a director of the compass needle, as to its utility, and as little more than a curiosity in science, is one of the mighty energies by which, instrumentally, the works of the great Creator are regulated; one of those subtle powers which he hath ordained as his servants, 'fulfilling his word;' and whereby, 'the sweet influences' of the whole system of the universe are bound together, controlled, and upheld. Thus the subject of magnetism becomes of the highest consideration; in science, as to its mightiness and extent of operation; and, in natural theology, as calculated to connect the researches of human intelligence, with him who hath created these wonders; to elevate the feelings of reverence and adoration in the devotional mind, and to proclaim more clearly, in proportion as the invisible things are understood, 'His eternal power and Godhead.'

"It is not my object, however, in this publication, to carry out those views to which the more enlarged consideration of magnetism, as a science, might be advantageously applied; but that, in thus showing something of the importance of the subject as a science, I may solicit, for the contributions which are here offered for it, such reasonable consideration as, in this connexion, they may fairly claim.

"To the subject of *magnetism*, my attention has, for a series of years, been more or less directed; latterly with the view, particularly, of producing more powerful instruments for the determination of delicate variations in, and the actual condition of, the earth's magnetism; a subject which, from its greatly increased importance, is now engaging attention in some of the principal observatories in Europe.

"In contemplating such improvement in instruments dependent for their adjustment on the earth's magnetism, the grand desideratum would obviously be, the attainment of increased energy, or directive power, in magnetic needles, or

bars of any given length or mass. And, that the attainment of such increased energy was a promising field of enquiry, I was satisfied, from the mere consideration of the surprising superiority in power of electro-magnets over permanent artificial magnets; strongly indicating the existence of a far greater capacity for magnetism, than we have hitherto been able to develop, or, if developed, to retain. Whilst this consideration yielded every encouragement to the enquiry, an experimental fact, in regard to the proportional power of magnets of *unequal thickness*, suggested that guidance, in pursuing the enquiry, which not only led (as will, I trust, subsequently appear) to a successful result in regard to the object particularly specified, but gave rise to investigations extending beyond my original design, and eminently calculated, I conceive, for the improvement of sea compasses—hitherto so very defective—as well as artificial magnets, and magnetic apparatus generally. The fact referred to was this. When examining, many years ago, the directive power of various artificial bar-magnets, for the purpose of determining the practicability of ascertaining the thickness of rocks, &c. in tunneling and mining, by the method of deviations communicated to the Royal Society in 1831,—the idea occurred to me, that, if the bars ordinarily employed for compass needles, &c. were divided into laminæ; or if, in other words, they were made up of thin plates to the extent of the masses of the bars commonly in use, a greater degree of energy would be obtained. Experiment fully justified this opinion. But previous to the application of the principle to instruments directed by the earth's magnetism, I had made trial of a combination of laminæ of *thoroughly tempered steel*, for the construction of a small compound magnet. The substance made use of was watch-spring, of which fourteen pieces, of two inches in length, were combined, after being magnetized, and formed a small magnet, weighing, in amount of steel, about one hundred grains. From a want of knowledge, at that time, of the best mode of magnetizing thin plates, the power obtained was much less than was expected; but when the same little instrument was subsequently magnetized, in a mass, by the process hereafter described, its efficiency became very striking,—the power being found to be such as to lift, by one pole, a polished nail of about 800 grains, or eight times its own weight.

“A trial apparatus, of the nature of a *variation needle*, on the same principle,—improved, however, for this purpose, by the *separation* of the plates—was constructed in the year 1836, which was exhibited to the ‘British Association for

the Promotion of Science,' the same year. But being without any *precise* knowledge of the laws of combination in magnetized plates, or even of the actual power of this instrument, though obviously great, a mere general idea only of its relative superiority could then be obtained. Since that time I have investigated, in an elaborate series of experiments, and with a somewhat expensive variety of apparatus, the principles of the construction adopted, so as satisfactorily to prove, I conceive, the decided advantage of that construction for sea, and other, compasses, and to apply the principle to various practical purposes in magnetics.

"In the original 'variation compass' just referred to, the plates, as I have intimated, were not placed in immediate contact, but separated by thin slips of wood or card-board; for I had previously found, when combining magnets for other purposes, that a material loss of power, in the individual intensities of the bars, was, in all cases, occasioned by combination: but that, when the combination was not made in contact, an inferior deterioration took place.

"The subjects to which I was primarily guided by these preliminary considerations and results, extended to the following particulars:—The effect of the division in various directions of the mass of steel, on its magnetic capabilities,—the law of combination of magnetized steel plates in contact—the law of combination when the plates are separated by limited spaces—the effect of temper or degree of hardness, and the degree of permanency of the power in combinations of magnetized steel plates. A few of the most important results obtained from these investigations, were forwarded to the Institute of France, in February, 1838. These investigations proved sufficient to show that the idea entertained in the outset, of practicability of producing, by means of combination of magnetized steel plates, more powerful apparatus than had hitherto been in use, for experiment and observation in magnetical science, and for practical purposes in magnetics generally, was not incorrectly founded. But the most important practical applications of these principles, with the results of several new and distinct investigations, yet remain behind. The description of these, in the first instance, is the object of the present publication; and it is hoped that the results will be found to develop some new and some improved principles of construction, applicable both to instruments designed to be directed by the earth's magnetism, and to the improvement of apparatus in which a permanent and concentrate energy are requisite; together with a useful application of some of the laws developed in these or previous investigations, to the

testing of the quality of steel, and the determining of the degree of its hardness, purposes of the highest importance in the construction of delicate instruments used in the arts, or by professional men.

“The several results and applications of these recent personal researches, may be conveniently classed under separate heads, belonging to the development of principles, or of different practical processes, in magnetism. Some of these will, no doubt, be resolvable, in certain particulars, into principles or methods heretofore known; but, in all, it is presumed, something peculiar, as to decisiveness of the results bearing on principles, or as to convenience of adaptation, or efficacy of manipulation, or improvement in construction, in regard to the practical subjects, will be found.”

From the above prefatory chapter, our readers will discover that the objects of Mr. Scoresby's investigations are of a high scientific character; and we can assure them that we have not met with so valuable a work on magnetics as that before us, since the appearance of the second edition of Mr. Barlow's “Magnetic Attractions,” a work of great merit, and intrinsic importance, in this branch of physics. It is such productions as these that every scientific man delights to peruse; and every scientific journalist ought to take a pleasure in recommending to his readers. Every novel fact that becomes developed by physical enquiries gives new impulses to the pursuit, implants an additional gem in the diadem of science, enhances the lustre of the whole, and, sooner or later, yields new sources of comfort and happiness to man.

X. MISCELLANEOUS ARTICLES.

Galvanic experiments on the body of an executed Murderer.

Coleman, a mulatto, who murdered his wife, was executed at New York on the 15th of Feb., 1839. After the body had hung for about a quarter of an hour it was cut down. Mr. Chilton, and several other scientific men, then operated in the following way on the corpse. The instrument used in these experiments was a newly invented one, called a Galvanic Multiplier; the whole amount of zinc surface exposed to the acid was about one foot, and yet the shock produced is equal, if not greater, than that of a battery of 100 inch plates.

1st Experiment.—The lungs were filled with oxygen gas. The phrenic nerve and eighth pair were dissected in the neck; a metallic piece, having a number of points on it, was placed

over the ribs, the points being inserted through the skin. The moment the lungs were filled with the gas, the galvanic current was passed from the nerves at the neck to the diaphragm. The object was to bring about respiration. The effect produced was, violent contraction of all the muscles, the chest heaved but no air appeared to enter the lungs, the head and neck were thrown on one side by the spasm produced. *2d.* The metallic piece was removed from the abdomen, and an incision was made through the cartilage of the seventh rib, one pole of the instrument was placed in the opening, so as to touch the diaphragm; the other was placed on the neck. The effect produced was similar to the first. *3d.* The posterior tibial nerve at the heel was exposed; one pole applied to this the other to the neck. Effect—the muscle of the leg was thrown into action, with convulsive movements of the body. *4th.* One pole was held at the tibian nerve—the mouth was then opened, and the other pole put into it. The moment it touched the tongue the teeth became firmly clenched, and held so hard on to the wire as to require considerable force to extricate it. This was repeated several times. *5th.* The next experiment was to try the effect produced by merely applying the poles of the instrument to the surface of the body, previously wetting its parts with a saline solution, to render the contact more perfect. The effects on the body appeared quite as great as when the large nerves were touched. The poles of the apparatus were placed in the above manner, one to the leg, the other to different parts of the face. The facial muscles were alternately thrown into action as the different nerves of the face were touched. The effect of this was terrific in the extreme. Every muscle of the grim murderer's countenance was thrown into the most horrible contortions: rage, horror, anguish, and despair, the most rapid smiles, the most hideous expressions of contempt and hatred, by turns were depicted on his countenance, and gave a fearful wildness to his face, which far surpassed even the most vivid imagination from Fuseli's brain, or Kean's scenic display that we ever witnessed. Several of the audience were excessively appalled; some left in double quick time, and many confessed, that, if they had staid, they certainly should have fainted. At one part of the operations, when the murderer raised his right arm and passed it in different directions, we saw the cheeks of several stout hearted fellows blanched with fear: and one, whose name we do not wish to mention, actually whispered "sure, he has come to life." Above an hour was spent in the experiments, and then the prison was cleared and the body removed under the directions of the surgeons.—*Jersey Times and Naval and Military Chronicle.*

On Thursday afternoon, the 13th of June, the Town of Croydon, in Surrey, was visited by a most tremendous storm of lightning and thunder. It commenced about 5 o'clock and raged with great violence for a considerable time. A young man, named William Mackrell, in the employ of Mr. Walters, of Ridley Oaks, being engaged in pitching some fold stakes and was about to leave his work, and shelter in the house, when he was struck by the electric fluid and fell to the ground quite dead. Some stakes which he held in his hand at the time were shivered to pieces. He was a fine strong man, about 30 years of age. A great deal of injury was done to property in the neighbourhood.

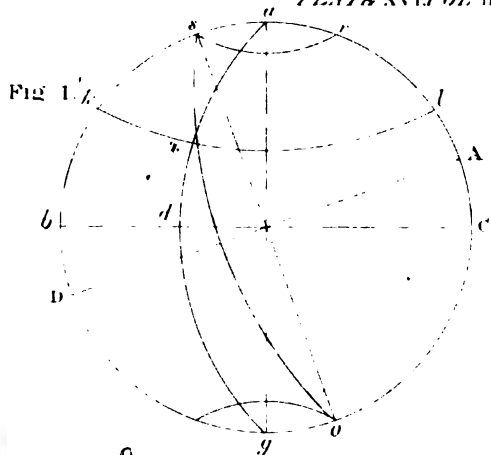
To obtain Potassium by voltaic action. "A thin piece" of hydrate of potassa "is placed between two discs of platina, connected with the extremities of a voltaic apparatus of 200 double plates; it will soon undergo fusion, oxygen will separate at the positive surface, and small globules will appear at the negative surface, which consist of potassium." I discovered this metal in the beginning of October 1807.—*Sir H. Davy.*

One hundred two-inch plates of a Cruickshank's battery decomposes the potassa very well. If the battery be too active the liberated potassium is apt to take fire. EDIT.

Amalgam for the Rubbers of Electrical Machines.

"The amalgam which I use is made by melting together one ounce of tin and two ounces of zinc, which are mixed whilst fluid, with six ounces of mercury, and agitated in an iron or wooden box until cold. It is then reduced to a fine powder in a mortar, and mixed with sufficient hog's lard to form a paste." *Singer.*

Fuse a small quantity of zinc either in a crucible or ladle, and pour it gently into about four times its weight of mercury, previously heated in a stone or iron mortar, and stir it well during the time with the warm pestle. Continue to rub the amalgam till quite cool, in order to incorporate the two metals completely; which if well performed, will give the amalgam a smooth butter-like consistence. It may be made softer, if necessary, by adding mercury during the process. This amalgam, mixed with a very little tallow, is the best we have yet used. EDIT.



part of Fig 4.

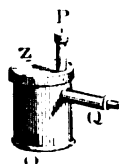
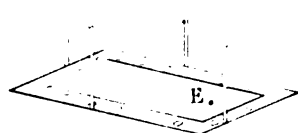


Fig 2.

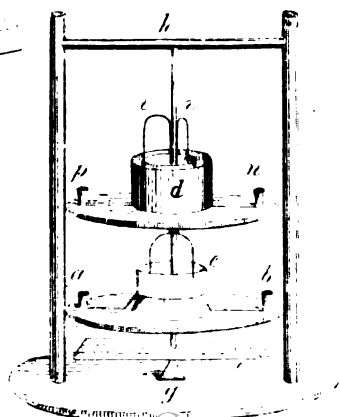


Fig 4.

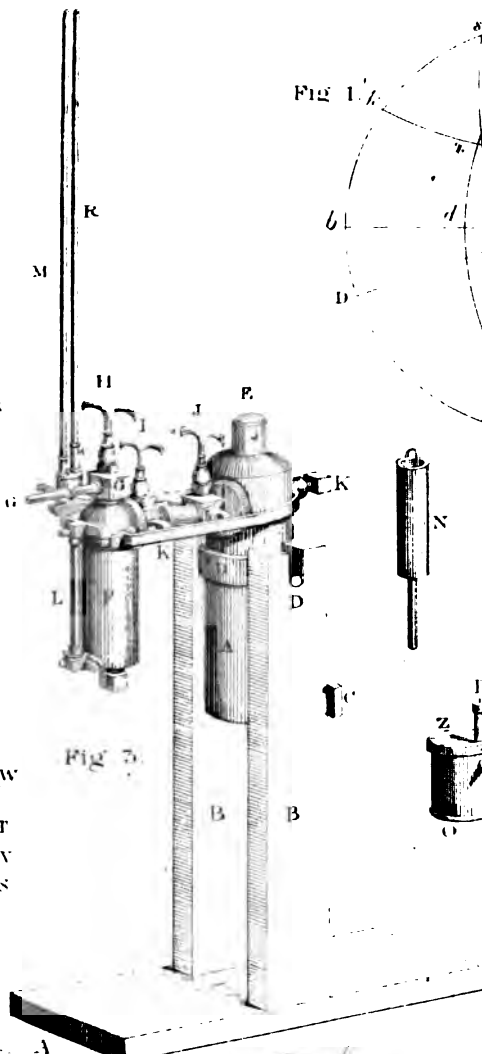
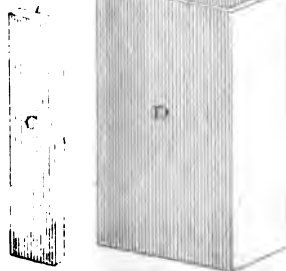


Fig 3.



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Fig 1

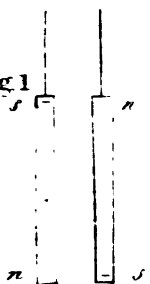


Fig. 5.



Fig 7

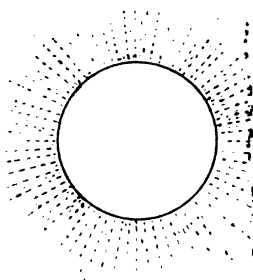


Fig. 9.

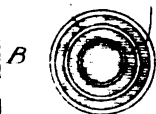
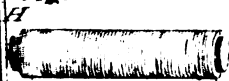


Fig 12.



Fig. 1.

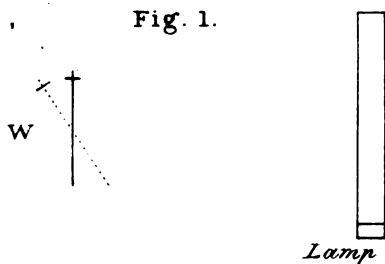


Fig. 2.

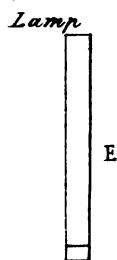
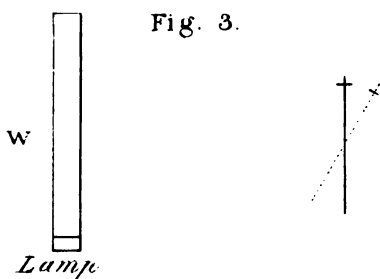


Fig. 3.



Lamp

Fig. 4.

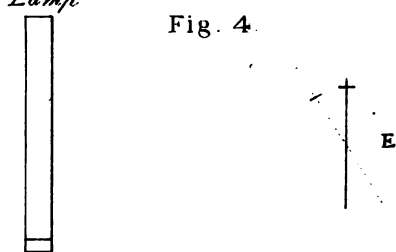


Fig. 5.

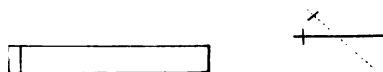


Fig. 6.

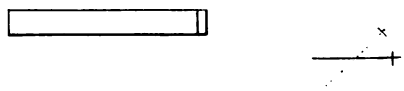


Fig. 7.

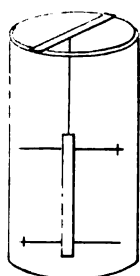


Fig. 8.

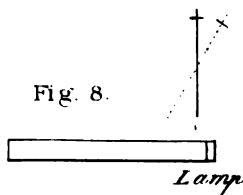


Fig. 9.

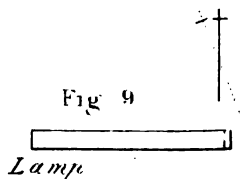


Fig. 10.

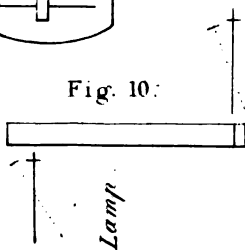
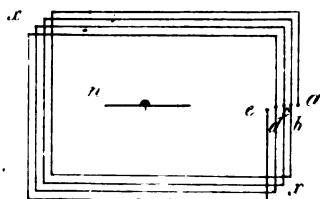


Fig. 11.



THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;

AND
Guardian of Experimental Science.

AUGUST, 1839.

XI. *Experimental Researches in Electricity.—Eleventh Series.* By MICHAEL FARADAY, Esq., D.C.L., F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburg, Florence, Copenhagen, Berlin, &c., &c.*

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(Continued from page 25.)

1234. One of the inductive apparatus already described (1187, &c.) had a hemispherical cup of shell lac introduced, which being in the interval between the inner ball and the lower hemisphere, nearly occupied the space there; consequently when the apparatus was charged, the lac was the dielectric or insulating medium through which the induction took place in that part. When this apparatus was first charged with electricity (1198.) up to a certain intensity, as 400°, measured by the Coulomb's electrometer (1180.), it sank much faster from that degree than if it had been previously charged to a higher point, and had gradually fallen to 400°; or than it would do if the charge were, by a second application, raised up again to 400°; all other things remaining the same. Again, if after having been charged for some time, as fifteen or twenty minutes, it was suddenly and perfectly discharged, even the stem having all electricity removed from it (1203.), then the apparatus being left to itself, would gradually recover a charge, which in nine or ten minutes would rise up to 50° or 60°, and in one instance to 80°.

1235. The electricity which in these cases returned from an apparently latent to a sensible state, was always of the

* From the Transactions of the Royal Society.

same kind as that which had been given by the charge. The return took place at both the inducing surfaces; for if after the perfect discharge of the apparatus the whole was insulated, as the inner ball resumed a positive state the outer sphere acquired a negative condition.

1236. This effect was at once distinguished from that produced by the excited stem acting in curved lines of induction (1203. 1232.), by the circumstance that all the returned electricity could be perfectly and instantly discharged. It appeared to depend upon the shell lac within, and to be, in some way, due to electricity evolved from it in consequence of a previous condition into which it had been brought by the charge of the metallic coatings or balls.

1237. To examine this state more accurately, the apparatus, with the hemispherical cup of shell lac in it, was charged for about forty-five minutes to above 600° with positive electricity at the balls *A* and *B* (fig. 1, Plate I.) above and within. It was then discharged, opened, the shell lac taken out, and its state examined; this was done by bringing the carrier ball near the shell lac, uninsulating it, insulating it, and then observing what charge it had acquired. As it would be a charge by induction, the state of the ball would indicate the opposite state of electricity in that surface of the shell lac which had produced it. At first the lac appeared quite free from any charge; but gradually its two surfaces assumed opposite states of electricity, the concave surface, which had been next the inner and positive ball, assuming a positive state, and the convex surface, which had been in contact with the negative coating, acquiring a negative state; these states gradually increasing in intensity for some time.

1238. As the return action was evidently greatest instantly after the discharge, I again put the apparatus together, and charged it for fifteen minutes as before, the inner ball positively. I then discharged it, instantly removing the upper hemisphere with the interior ball, and, leaving the shell lac cup in the lower uninsulated hemisphere, examined its inner surface by the carrier ball as before (1237.). In this way I found the surface of the shell lac actually *negative*, or in the reverse state to the ball which had been in it; this state quickly disappeared, and was succeeded by a positive condition, gradually increasing in intensity for some time, in the same manner as before. This first negative condition of the surface opposite the positive charging ball is a natural consequence of the state of things, the charging ball being in contact with the shell lac only in a few points. It does not interfere with the general result and peculiar state now under

consideration, except that it assists in illustrating in a very marked manner the ultimate assumption by the surfaces of the shell lac of an electrified condition, similar to that of the metallic surfaces opposed to or against them.

1239. *Glass* was then examined with respect to its power of assuming this peculiar state. I had a thick flint glass hemispherical cup formed, which would fit easily into the space *o* of the lower hemisphere (1188. 1189.); it had been heated and varnished with a solution of shell lac in alcohol for the purpose of destroying the conducting power of the vitreous surface. Being then well warmed and experimented with, I found it could also assume the *same state*, but not apparently to the same degree, the return action amounting in different cases to quantities from 6° to 18° .

1240. *Spermaceti* experimented with in the same manner gave striking results. When the original charge had been sustained for fifteen or twenty minutes at about 500° , the return charge was equal to 95° or 100° , and was about fourteen minutes arriving at the maximum effect. A charge continued for not more than two or three seconds was here succeeded by a return charge of 50° or 60° . The observations formerly made (1234.) held good with this substance. *Spermaceti*, though it will insulate a low charge for some time, is a better conductor than shell lac, glass, and sulphur; and this conducting power is connected with its readiness in exhibiting the particular effect under consideration.

1241. *Sulphur*.—I was anxious to obtain the amount of effect with this substance, first, because it is an excellent insulator, and in that respect would illustrate the relation of the effect to the degree of conducting power possessed by the dielectric (1247.); and in the next place, that I might obtain that body giving the smallest degree of the effect now under consideration, for the investigation of the question of specific inductive capacity (1277.).

1242. With a good hemispherical cup of sulphur cast solid and sound, I obtained the return charge, but only to an amount of 17° or 18° . Thus glass and sulphur, which are bodily very bad conductors of electricity, and indeed almost perfect insulators, gave very little of this return charge.

1243. I tried the same experiment having *air* only in the inductive apparatus. After a continued high charge for some time I could obtain a little effect of return action, but it was ultimately traced to the shell lac of the stem.

1244. I sought to produce something like this state with one electric power and without induction; for upon the theory of an electric fluid or fluids, that did not seem impossible,

and then I should have obtained an absolute charge (1169. 1177.), or something equivalent to it. In this I could not succeed. I excited the outside of a cylinder of shell lac very highly for some time, and then quickly discharging it (1203.), waited and watched whether any return charge would appear, but such was not the case. This is another fact in favour of the inseparability of the two electric forces, and another argument for the view that induction and its concomitant phenomena depend upon a polarity of the particles of matter.

1245. Although inclined at first to refer these effects to a peculiar masked condition of a certain portion of the forces, I think I have since correctly traced them to known principles of electrical action. The effects appear to be due to an actual penetration of the charge to some distance within the electric, at each of its two surfaces, by what we call conduction; so that, to use the ordinary phrase, the electric forces sustaining the induction are not upon the metallic surfaces only, but upon and within the dielectric also, extending to a greater or smaller depth from the metal linings. Let c (fig. 10.) be the section of a plate of any dielectric, a and b being the metallic coatings; let b be uninsulated, and a be charged positively; after ten or fifteen minutes, if a and b be discharged, insulated, and immediately examined, no electricity will appear in them; but in a short time, upon a second examination, they will appear charged in the same way, though not to the same degree, as they were at first. Now suppose that a portion of the positive force has, under the coercing influence of all the forces concerned, penetrated the dielectric and taken up its place at the line p , a corresponding portion of the negative force having also assumed its position at the line n ; that in fact the electric at these two parts has become charged positive and negative; then it is clear that the induction of these two forces will be much greater one towards the other, and less in an external direction, now that they are at the small distance np from each other, than when they were at the larger interval ab . Then let a and b be discharged; the discharge destroys or neutralizes all external induction, and the coatings are therefore found by the carrier ball un-electrified; but it also removes almost the whole of the forces by which the electric charge was driven into the dielectric, and though probably a part goes forward in its passage and terminates in what we call discharge, the greater portion returns on its course to the surfaces of c , and consequently to the conductors a and b , and constitutes the re-charge observed.

1246. The following is the experiment on which I rest for

the truth of this view. Two plates of spermaceti, *d* and *f* (fig. 11.), were put together to form the dielectric, *a* and *b* being the metallic coatings of this compound plate, as before. The system was charged, then discharged, insulated, examined, and found to give no indications of electricity to the carrier ball. The plates *d* and *f* were then separated from each other, and instantly *a* with *d* was found in a positive state, and *b* with *f* in a negative state, nearly all the electricity being in the linings *a* and *b*. Hence it is clear that, of the forces sought for, the positive was in one half of the compound plate and the negative in the other half; for when removed bodily with the plates from each other's inductive influence, they appeared in separate places, and resumed of necessity their power of acting by induction on the electricity of surrounding bodies. Had the effect depended upon a peculiar relation of the contiguous particles of matter only, then each half plate, *d* and *f*, should have shown positive force on one surface and negative on the other.

1247. Thus it would appear that the best solid insulators, such as shell lac, glass, and sulphur, have conductive properties to such an extent, that electricity can penetrate them bodily, though always subject to the overruling condition of induction (1178.). As to the depth to which the forces penetrate in this form of charge of the particles, theoretically, it should be throughout the mass, for what the charge of the metal does for the portion of dielectric next to it, should be done by the charged dielectric for the portion next beyond it again; but probably in the best insulators the sensible charge is to a very small depth only in the dielectric, for otherwise more would disappear in the first instance whilst the original charge is sustained, less time would be required for the assumption of the particular state, and more electricity would re-appear as return charge.

1248. The condition of *time* required for this penetration of the charge is important, both as respects the general relation of the cases to conduction, and also the removal of an objection that might otherwise properly be raised to certain results respecting specific inductive capacities, hereafter to be given (1269. 1277.).

1249. It is the assumption for a time of this charged state of the glass between the coatings in the Leyden jar, which gives origin to a well-known phenomenon, usually referred to the diffusion of electricity over the uncoated portion of the glass, namely, the *residual charge*. The extent of charge which can spontaneously be recovered by a large battery, after perfect uninsulation of both surfaces, is very considerable, and

by far the largest portion of this is due to the return of electricity in the manner described. A plate of shell lac six inches square, and half an inch thick, or a similar plate of spermaceti an inch thick, being coated on the sides with tin-foil as a Leyden arrangement, will show this effect exceedingly well.

1250. The peculiar condition of dielectrics which has now been described, is evidently capable of producing an effect interfering with the results and conclusions drawn from the use of the two inductive apparatus, when shell lac, glass, &c. are used in one or both of them (1192. 1207.); for upon dividing the charge in such cases according to the method described (1198. 1207.), it is evident that the one just receiving its half charge must fall faster in its tension than the other. For suppose app. i. first charged, and app. ii. used to divide with it; though both may actually lose alike, yet app. i., which has been diminished one half, will be sustained by a certain degree of return action or charge (1234.), whilst app. ii. will sink the more rapidly from the coming on of the particular state. I have endeavoured to avoid this interference by performing the whole process of comparison as quickly as possible, and taking the force of app. ii. immediately after the division, before any sensible diminution of the tension arising from the assumption of the peculiar state could be produced; and I have assumed that as about three minutes pass between the first charge of app. i. and the division, and three minutes between the division and discharge, when the force of the non-transferable electricity is measured, the contrary tendencies for those periods would keep that apparatus in a moderately steady and uniform condition for the latter portion of time.

1251. The particular action described occurs in the shell lac of the stems, as well as in the *dielectric* used within the apparatus. It therefore constitutes a cause by which the outside of the stems may in some operations become charged with electricity, independent of the action of dust or carrying particles (1203.).

¶ v. *On specific Induction, or Specific inductive Capacity.*

1252. I now proceed to examine the great question of specific inductive capacity, i. e. whether different dielectric bodies actually do possess any influence over the degree of induction which takes place through them. If any such difference should exist, it appeared to me not only of high importance in the further comprehension of the laws and results of induction, but an additional and very powerful

argument for the theory I have ventured to put forth, that the whole depends upon a molecular action, in contradistinction to one at sensible distances.

The question may be stated thus: suppose A an electrified plate of metal suspended in the air, and B and C two exactly similar plates, placed parallel to and on each side of A at equal distances and uninsulated; A will then induce equally towards B and C. If in this position of the plates some other dielectric than air, as shell lac, be introduced between A and C, will the induction between them remain the same? Will the relation of C and B to A be unaltered, notwithstanding the difference of the dielectrics interposed between them?

1253. As far as I recollect, it is assumed that no change will occur under such variation of circumstances, and that the relations of B and C to A depend entirely upon their distance. I only remember one experimental illustration of the question, and that is by Coulomb,* in which he shows that a wire surrounded by shell lac took exactly the same quantity of electricity from a charged body as the same wire in air. The experiment offered to me no proof of the truth of the supposition, for it is not the mere films of dielectric substances surrounding the charged body which have to be examined and compared, but the *whole mass* between that body and the surrounding conductors at which the induction terminates. Charge depends upon induction (1171. 1178.); and if induction relate to the particles of the surrounding dielectric, then it relates to *all* the particles of that dielectric inclosed by the surrounding conductors, and not merely to the few situated next to the charged body. Whether the difference I sought for existed or not, I soon found reason to doubt the conclusion that might be drawn from Coulomb's result, and therefore had the apparatus made, which, with its use, has been already described (1187, &c.), and which appears to me well suited for the investigation of the question.

1254. Glass, and many bodies which might at first be considered as very fit to test the principle, proved exceedingly unfit for that purpose. Glass, principally in consequence of the alkali it contains, however well warmed and dried it may be, has a certain degree of conducting power upon its surface, dependent upon the moisture of the atmosphere, which renders it unfit for a test experiment. Resin, wax, naphtha, oil of turpentine, and many other substances were in turn rejected, because of a slight degree of conducting power pos-

* *Mémoires de l'Académie*, 1787, pp. 452, 453.

sessed by them ; and ultimately shell lac and sulphur were chosen, after many experiments, as the dielectrics best fitted for the investigation. No difficulty can arise in perceiving how the possession of a feeble degree of conducting power tends to make a body produce effects, which would seem to indicate that it had a greater capability of allowing induction through it than another body perfect in its insulation. This source of error has been the one I have found most difficult to obviate in the proving experiments.

1255. *Induction through Shell lac.*—As a preparatory experiment, I first ascertained generally that when a part of the surface of a thick plate of shell lac was excited or charged, there was no sensible difference in the character of the induction sustained by that charged part, whether exerted through the air in the one direction, or through the shell lac of the plate in the other : provided the second surface of the plate had not, by contact with conductors, the action of dust, or any other means, become charged (1203.). Its solid condition enabled it to retain the excited particles in a permanent position, but that appeared to be all ; for these particles acted just as freely through the shell lac on one side as through the air on the other. The same general experiment was made by attaching a disc of tin foil to one side of the shell lac plate, and electrifying it, and the results were the same. Scarcely any other solid substance than shell lac and sulphur, and no liquid substance that I have tried, will bear this examination. Glass in its ordinary state utterly fails ; yet it was essentially necessary to obtain this prior degree of perfection in the dielectric used, before any further progress could be made in the principal investigation.

1256. *Shell lac and air* were compared in the first place. For this purpose a thick hemispherical cup of shell lac was introduced into the lower hemisphere of one of the inductive apparatus (1187, &c.), so as nearly to fill the lower half of the space *o, o, o, o*, (fig. 1.) between it and the inner ball ; and then charges were divided in the manner already described (1198. 1207.), each apparatus being used in turn to receive the first charge before its division by the other. As the apparatus were known to have equal inductive power when air was in both (1209. 1211.), any differences resulting from the introduction of the shell lac would show a peculiar action in it, and if unequivocally referable to a specific inductive influence, would establish the point sought to be sustained. I have already referred to the precautions necessary in making the experiments (1199, &c.) ; and with respect to the error which might be introduced by the assumption of the peculiar state,

90 Dr. Faraday's *experimental researches in electricity.*

and that when induction through shell lac was converted into induction through air, the force or tension of the whole ought to be *increased*. The app. i. was therefore charged in the first place, and its force divided with app. ii. The following were the results:

App. i.	Lac.	App. ii.	Air.
			0°
215°		
204		
	Charge divided.		
	118	
118		
		0 after being discharged.
0		after being discharged.

1261. Here 204° must be the utmost of the divisible charge. The app. i. and ii. present 118° as their respective forces; both now much *above* the half of the first force, or 102°, whereas in the former case they were below it. The lac app. i. has lost only 86°, yet it has given to the air app. ii. 118°, so that the lac still appears much to surpass the air, the capacity of the lac app. i. to the air app. ii. being as 1·37 to 1.

1262. The difference of 1·55 and 1·37 as the expression of the capacity for the induction of shell lac seems considerable, but is in reality very admissible under the circumstances, for both are in error in *contrary directions*. Thus in the last experiment the charge fell from 215° to 204° by the joint effects of dissipation and absorption (1192. 1250.), during the time which elapsed in the electrometer operations, between the applications of the carrier ball required to give those two results. Nearly an equal time must have elapsed between the application of the carrier which gave the 204° result, and the division of the charge between the two apparatus; and as the fall in force progressively decreases in amount (1192.), if in this case it be taken at 6° only, it will reduce the whole transferable charge at the time of division to 198° instead of 204°; this diminishes the loss of the shell lac charge to 80° instead of 86°; and then the expression of specific capacity for it is increased, and, instead of 1·37°, is 1·47 times that of air.

1263. Applying the same correction to the former experiment in which air was *first* charged, the result is of the *contrary* kind. No shell lac hemisphere was then in the apparatus, and therefore the loss would principally be from dissipation, and not from absorption; hence it would be nearer to the degree of loss shown by the numbers 304° and 297°, and

being assumed as 6° would reduce the divisible charge to 284° . In that case the air would have lost 170° , and communicated only 113° to the shell lac; and the relative specific capacity of the latter would appear to be 1.50, which is very little indeed removed from 1.47, the expression given by the second experiment when corrected in the same way.

1264. The shell lac was then removed from app. i. and put into app. ii. and the experiments of division again made. I give the results, because I think the importance of the point justifies and even requires them.

App. i. Air.	Balls 200° .	App. ii. Lac.
		0°
286°	
233	
	Charge divided.	
		110
109	
		0.25 after discharge.
Trace	after discharge.

Here app. i. retained 109° , having lost 174° in communicating 110° to app. ii.; and the capacity of the air app. is to the lac app., therefore, as 1 to 1.58. If the divided charge be corrected for an assumed loss of only 3° , being the amount of previous loss in the same time, it will make the capacity of the shell lac app. 1.55 only.

1265. Then app. ii. was charged, and the charge divided thus:

App. i. Air.	App. ii. Lac.
0°	
.	256°
.	251
	Charge divided.
146
	149
a little
	after discharge.
.	a little after discharge.

Here app. i. acquired a charge of 146° , while app. ii. lost only 102° in communicating that amount of force; the capacities being, therefore, to each other as 1 to 1.43. If the whole transferable charge be corrected for a loss of 4° previous to division, it gives the expression of 1.49 for the capacity of the shell lac apparatus.

1266. These four expressions of 1.47, 1.50, 1.55, and 1.49 for the power of the shell lac apparatus, through the different

variations of the experiment, are very near to each other; the average is close upon 1.5, which may hereafter be used as the expression of the result. It is a very important result; and showing for this particular piece of shell lac a decided superiority over air in allowing or causing the act of induction, it proved the growing necessity of a more close and rigid examination of the whole question.

1267. The shell lac was of the best quality, and had been carefully selected and cleaned; but as the action of any conducting particles in it would tend, virtually, to diminish the quantity or thickness of the dielectric used, and produce effects as if the two inducing surfaces of the conductors in that apparatus were nearer together than in the one with air only, I prepared another shell lac hemisphere, of which the material had been dissolved in strong spirit of wine, the solution filtered, and then carefully evaporated. This is not an easy operation, for it is difficult to drive off the last portions of alcohol without injuring the lac by the heat applied; and unless they be dissipated, the substance left conducts too well to be used in these experiments. I prepared two hemispheres this way, one of them unexceptionable; and with it I repeated the former experiments with all precautions. The results were exactly of the same kind; the following expressions for the capacity of the shell lac apparatus, whether it were app. i. or ii., being given directly by the experiments 1.46, 1.50, 1.52, 1.51; the average of these and several others being very nearly 1.5.

1268. As a final check upon the general conclusion, I then actually brought the surfaces of the air apparatus, corresponding to the place of the shell lac in its apparatus, nearer together, by putting a metallic lining into the lower hemisphere of the one not containing the lac (1213.). The distance of the metal surface from the carrier ball was in this way diminished from 0.62 of an inch to 0.435 of an inch, whilst the interval occupied by the lac in the other apparatus remained 0.62 of an inch as before. Notwithstanding this change, the lac apparatus showed its former superiority; and whether it or the air apparatus was charged first, the capacity of the lac apparatus to the air apparatus was by the experimental results as 1.45 to 1.

1269. From all the experiments I have made, and their constant results, I cannot resist the conclusion that shell lac does exhibit a case of *specific inductive capacity*. I have tried to check the trials in every way, and if not remove, at least estimate, every source of error. That the final result is not due to common conduction is shown by the capability of the

apparatus to retain the communicated charge; that it is not due to the conductive power of inclosed small particles, by which they could acquire a polarized condition as conductors, is shown by the effects of the shell lac purified by alcohol; and, that it is not due to any influence of the charged state, formerly described (1250.), first absorbing and then evolving electricity, is indicated by the *instantaneous* assumption and discharge of those portions of the power which are concerned in the phenomena, that effect occurring in these cases, as in all others of ordinary induction by charged conductors. The latter argument is the more striking in the case where the air apparatus is employed to divide the charge with the lac apparatus, for it obtains its portion of electricity in an *instant*, and yet is charged far above the *mean*.

1270. Admitting for the present the general fact sought to be proved; then 1·5, though it expresses the capacity of the apparatus containing the hemisphere of shell lac, by no means expresses the relation of lac to air. The lac only occupies one half of the space *o, o*, of the apparatus containing it, through which the induction is sustained: the rest is filled with air, as in the other apparatus; and if the effect of the two upper halves of the globes be abstracted, then the comparison of the shell lac powers in the lower half of the one, with the power of the air in the lower half of the other, will be as 2 : 1; and even this must be less than the truth, for the induction of the upper part of the apparatus, i.e. of the wire and ball B (fig. 1.) to external objects, must be the same in both, and considerably diminish the difference dependent upon, and really producible by, the influence of the shell lac within.

1271. *Glass*.—I next worked with glass as the dielectric. It involved the possibility of conduction on its surface, but it excluded the idea of conducting particles within its substance (1267.) other than those of its own mass. Besides this it does not assume the charged state (1239.) so readily, or to such an extent as shell lac.

1272. A thin hemispherical cup of glass being made hot was covered with a coat of shell lac dissolved in alcohol, and after being dried for many hours in a hot place, was put into the apparatus and experimented with. It exhibited effects so slight, that though they were in the direction indicating a superiority of glass over air, they were allowed to pass as possible errors of experiment; and the glass was considered as producing no sensible effect.

1273. I then procured a thick flint glass hemispherical cup resembling that of shell lac (1239.), but not filling up the

space *o, o*, so well. Its average thickness was 0.4 of an inch, there being an additional thickness of air, averaging 0.22 of an inch, to make up the whole space of 0.62 of an inch between the inducing metallic surfaces. It was covered with a film of shell lac as the former was, (1272.) and being made very warm, was introduced into the apparatus, also warmed, and experiments made with it as in the former instances (1257. &c.). The general results were the same as with shell lac, i.e. glass surpassed air in its power of favouring induction through it. The two best results as respected the state of the apparatus for retention of charge, &c., gave, when the air apparatus was charged first 1.336, and when the glass apparatus was charged first 1.45, as the specific inductive capacity for glass, both being without correction. The average of nine results, four with the glass apparatus first charged, and five with the air apparatus first charged, gave 1.38 as the power of the glass apparatus; 1.22 and 1.46 being the minimum and maximum numbers with all the errors of experiment upon them. In all the experiments the glass apparatus took up its inductive charge instantly, and lost it as readily; and during the short time of each experiment, acquired the peculiar state in a small degree only, so that the influence of this state, and also of conduction upon the results, must have been small.

1274. Allowing specific inductive capacity to be proved and active in this case, and 1.38 as the expression for the glass apparatus, then the specific inductive capacity of flint glass will be above 1.76, not forgetting that this expression is for a piece of glass of such thickness as to occupy not quite two-thirds of the space through which the induction is sustained (1273. 1253.).

1275. *Sulphur*.—The same hemisphere of this substance was used in app. ii. as was formerly referred to (1242.). The experiments were well made, i.e. the sulphur itself was free from charge both before and after each experiment, and no action from the stem appeared (1203. 1232.), so that no correction was required on that score. The following are the results when the air apparatus was first charged and divided :

App. i. Air.	App. ii. Sulphur.
Balls 280°	
0°	0°
438	
434	

App. i.	Air.	App. ii.	Sulphur.
	Charge divided.		
	.	.	162
164	.	.	
	.	.	160
162	.	.	
	.	.	0. after discharge.
0	.	.	after discharge.

Here app. i. retained 164°, having lost 270° in communicating 162° to app. ii., and the capacity of the air apparatus is to that of the sulphur apparatus as 1 to 1.66.

1276. Then the sulphur apparatus was charged first, thus:

App. i.	App. ii.
	0°
0°	.
	.
	.
	395
	388
	Charge divided.
237	.
	.
	.
	238
0	.
	.
	0 after discharge.
	after discharge.

Here app. ii. retained 238°, and gave up 150° in communicating a charge of 237° to app. i., and the capacity of the air apparatus is to that of the sulphur apparatus as 1 to 1.58. These results are very near to each other, and we may take the mean 1.62 as representing the specific inductive capacity of the sulphur apparatus; in which case the specific inductive capacity of sulphur itself is compared to air = 1 (1270.) will be about or above 2.24.

1277. This result with sulphur I consider as one of the most unexceptionable. The substance when fused was perfectly clear, pellucid, and free from particles of dirt (1267.), so that no interference of small conducting particles confused the result. The body when solid is an excellent insulator, and by experiment was found to take up, with great slowness, that state (1241. 1242.) which alone seemed likely to disturb the conclusion. The experiments themselves, also, were free from any need of correction. Yet notwithstanding these circumstances, so favourable to the exclusion of error, the result is a higher specific inductive capacity for sulphur than for any other body as yet tried; and though this may in part be due to the sulphur being in a better shape, i. e. filling up more completely the space *o, o*, (fig. 1.) than the cups of shell-lac and glass, still I feel satisfied that the experiments altogether

fully prove the existence of a difference between dielectrics as to their power of favouring an inductive action through them; which difference may, for the present, be expressed by the term *specific inductive capacity*.

1278. Having thus established the point in the most favourable cases that I could anticipate, I proceeded to examine other bodies amongst solids, liquids, and gases. These results I shall give with all convenient brevity.

1279. *Spermaceti*.—A good hemisphere of spermaceti being tried as to conducting power whilst its two surfaces were still in contact with the tin-foil moulds used in forming it, was found to conduct sensibly even whilst warm. On removing it from the moulds and using it in one of the apparatus, it gave results indicating a specific inductive capacity between 1.3 and 1.6 for the apparatus containing it. But as the only mode of operation was to charge the air apparatus, and then after a quick contact with the spermaceti apparatus, ascertain what was left in the former (1231.), no great confidence can be placed in the results. They are not in opposition to the general conclusion, but cannot be brought forward as argument in favour of it.

1280. I endeavoured to find some liquids which would insulate well, and could be obtained in sufficient quantity for these experiments. Oil of turpentine, native naphtha rectified, and the condensed oil gas fluid, appeared by common experiments to promise best as to insulation. Being left in contact with fused carbonate of potassa, chloride of lime, and quick lime for some days and then filtered, they were found much injured in insulating power; but after distillation acquired their best state, though even then they proved to be conductors when large metallic contact was made with them.

1281. *Oil of Turpentine rectified*.—I filled the lower half of app. i. with the fluid; and as it would not hold a charge sufficiently to enable me first to measure and then divide it, I charged app. ii. containing air, and dividing its charge with app. i. by a quick contact, measured that remaining in app. ii.: for, theoretically, if a quick contact would divide up to equal tension between the two apparatus, yet without sensible loss from the conducting power of app. i.; and app. ii. were left charged to a degree of tension above half the original charge, it would indicate that oil of turpentine had less specific inductive capacity than air; or, if left charged below that mean state of tension, it would imply that the fluid had the greater inductive capacity. In an experiment of this kind, app. ii. gave as its charge 390° before division with app. i., and 175° afterwards, which is less than the half of 390° . Again, being

at 175° before division, it was 79° after, which is also less than half the divided charge. Being at 79°, it was a third time divided, and then fell to 36°, less than the half of 79°. Such are the best results I could obtain; they are not inconsistent with the belief that oil of turpentine has a greater specific capacity than air, but they do not prove the fact, since the disappearance of more than half the charge may be due to the conducting power merely of the fluid.

1282. *Naphtha*.—This liquid gave results similar in their nature and direction to those with oil of turpentine.

1283. A most interesting class of substances, in relation to specific inductive capacity, now came under review, namely, the gases or æriform bodies. These are so peculiarly constituted, and are bound together by so many striking physical and chemical relations, that I expected some remarkable results from them: air in various states was selected for the first experiments.

1284. *Air, rare and dense*.—Some experiments of division (1208.) seemed to show that dense and rare air were alike in the property under examination. A simple and better process was to attach one of the apparatus to an air pump, to charge it, and then examine the tension of the charge when the air within was more or less rarefied. Under these circumstances it was found, that commencing with a certain charge, that charge did not change in its tension or force as the air was rarefied, until the rarefaction was such that *discharge* across the space *o, o* (fig. 1.) occurred. This discharge was proportionate to the rarefaction; but having taken place, and lowered the tension to a certain degree, that degree was not at all affected by restoring the pressure and density of the air to their first quantities.

Inches of Mercury.

Thus at a pressure of	30	the charge was	88°
Again	30	the charge was	88
Again	30	the charge was	87
Reduced to	14	the charge was	87
Raised again to	30	the charge was	86
Being now reduced to	3·4	the charge fell to	81
Raised again to	30	the charge was still	81

1295. The charges were low in these experiments, first that they might not pass off at low pressure, and next that little loss by dissipation might occur. I now reduced them still lower, that I might rarefy further, and for this purpose in the following experiment used a measuring interval in the electrometer of only 15° (1185.). The pressure of air within

the apparatus being reduced to 1.9 inches of mercury, the charge was found to be 29° ; then letting in air till the pressure was 30 inches, the charge still 29° .

1286. These experiments were repeated with pure oxygen with the same consequences.

1287. This result of *no variation* in the electric tension being produced by variation in the density or pressure of the air, agrees perfectly with those obtained by Mr. Harris, and described in his beautiful and important investigations contained in the Philosophical Transactions*; namely that induction is the same in rare and dense air, and that the divergence of an electrometer under such variations of the air continues the same, provided no electricity pass away from it. The effect is one entirely independent of that power which dense air has of causing a higher charge to be retained upon the surface of conductors in it than can be retained by the same conductors in rare air; a point I propose considering hereafter.

1288. I then compared *hot and cold air* together, by raising the temperature of one of the inductive apparatus as high as it could be without injury, and then dividing charges between it and the other apparatus containing cold air. The temperatures were about 50° and 200° . Still the power or capacity appeared to be unchanged; and when I endeavoured to vary the experiment, by charging a cold apparatus and then warming it by a spirit lamp, I could obtain no proof that the inductive capacity underwent any alteration.

1289. I compared *damp and dry air* together, but could find no difference in the results.

1290. *Gases*.—A very long series of experiments was then undertaken for the purpose of comparing *different gases* one with another. They were all found to insulate well, except such as acted on the shell lac of the supporting stem; these were chlorine, ammonia, and muriatic acid. They were all dried by appropriate means before being introduced into the apparatus. It would have been sufficient to have compared each with air; but, in consequence of the striking result which came out, namely, that *all had the same power of, or capacity for*, sustaining induction through them, (which perhaps might have been expected after it was found that no variation of density or pressure produced any effect,) I was induced to compare them, experimentally, two and two in various ways, that no difference might escape me, and that the sameness of result might stand in full opposition to the contrast of pro-

* Philosophical Transactions, 1834, pp. 223, 224, 237, 244. [See L. and E. Phil. Mag. vol. iv. p. 436.—EDIT.]

perty, composition, and condition which the gases themselves presented.

1291. The experiments were made upon the following pairs of gases.

1. Nitrogen and	Oxygen.
2. Oxygen	Air.
3. Hydrogen	Air.
4. Muriatic acid gas ..	Air.
5. Oxygen	Hydrogen.
6. Oxygen	Carbonic acid.
7. Oxygen	Olefiant gas.
8. Oxygen	Nitrous gas.
9. Oxygen	Sulphurous acid.
10. Oxygen	Ammonia.
11. Hydrogen	Carbonic acid.
12. Hydrogen	Olefiant gas.
13. Hydrogen	Sulphurous acid.
14. Hydrogen	Fluo-silicic acid.
15. Hydrogen	Ammonia.
16. Hydrogen	Arseniuretted hydrogen.
17. Hydrogen	Sulphuretted hydrogen.
18. Nitrogen.....	Olefiant gas.
19. Nitrogen.....	Nitrous gas.
20. Nitrogen.....	Nitrous oxide.
21. Nitrogen.....	Ammonia.
22. Carbonic oxide	Carbonic acid.
23. Carbonic oxide	Olefiant gas.
24. Nitrous oxide.....	Nitrous gas.
25. Ammonia	Sulphurous acid.

1292. Notwithstanding the striking contrasts of all kinds which these gases present of property, of density, whether simple or compound, anions or cations (665.), of high or low pressure (1284. 1286.), hot or cold (1288.), not the least difference in their capacity to favour or admit electrical induction through them could be perceived. Considering the point established, that in all these gases induction takes place by an action of contiguous particles, this is the more important, and adds one to the many striking relations which hold between bodies having the gaseous condition and form. Another equally important electrical relation, which will be examined in the next paper, is that which the different gases have to each other at the *same pressure* of causing the retention of the *same or different degrees of charge* upon conductors in them. These two results appear to bear importantly upon the subject of electro-chemical excitation and decomposition;

for as all these phenomena, different as they seem to be, must depend upon the electrical forces of the particles of matter, the very distance at which they seem to stand from each other will do much, if properly considered, to illustrate the principle by which they are held in one common bond, and subject, as they must be, to one common law.

1293. It is just possible that the gases may differ from each other in their specific inductive capacity, and yet by quantities so small as not to be distinguished in the apparatus I have used. It must be remembered, however, that in the gaseous experiments the gases occupy all the space *o.o.* (fig. 1.) between the inner and outer ball, except the small portion filled by the stem; and the results, therefore, are twice as delicate as those with solid dielectrics.

1294. The insulation was good in all the experiments recorded, except Nos. 10, 15, 21, and 25, being those in which ammonia was compared with other gases. When shell lac is put into ammoniacal gas its surface gradually acquires conducting power, and in this way the lac part of the stem within was so altered, that the ammonia apparatus could not retain a charge with sufficient steadiness to allow of division. In these experiments, therefore, the other apparatus was charged; its charge measured and divided with the ammonia apparatus by a quick contact, and what remained untaken away by the division again measured (1281.). It was so nearly one half of the original charge, as to authorize, with this reservation, the insertion of ammoniacal gas amongst the other gases, as having equal power with them.

1295. Thus *induction* appears to be essentially an action of contiguous particles, through the intermediation of which the electric force, originating or appearing at a certain place, is propagated to or sustained at a distance, appearing there as a force of the same kind exactly equal in amount, but opposite in its direction and tendencies (1164.). Induction requires no sensible thickness in the conductors which may be used to limit its extent; an uninsulated leaf of gold may be made very highly positive on one surface, and as highly negative on the other, without the least interference of the two states whilst the inductions continue. Nor is it affected by the nature of the limiting conductors, provided time be allowed, in the case of those which conduct slowly, for them to assume their final state (1170.).

1296. But with regard to the *dielectrics* or insulating media, matters are very different (1167.). Their thickness has an immediate and important influence on the degree of induction. As to their quality, though all gases and vapours are

alike, whatever their state, amongst solid bodies, and between them and gases, there are differences which prove the existence of *specific inductive capacities*, these differences being in some cases very great.

1297. The direct inductive force, which may be conceived to be exerted in lines between the two limiting and charged conducting surfaces, is accompanied by a lateral or transverse force equivalent to a dilatation or repulsion of these representative lines (1224.): or the attractive force which exists amongst the particles of the dielectric in the direction of the induction is accompanied by a repulsive or a diverging force in the transverse direction (1304.).

1298. Induction appears to consist in a certain polarized state of the particles, into which they are thrown by the electrified body sustaining the action, the particles assuming positive and negative points or parts, which are symmetrically arranged with respect to each other and the inducing surfaces or particles.* The state must be a forced one, for it is originated and sustained only by force, and sinks to the normal or quiescent state when that force is removed. It can be *continued* only in insulators by the same portion of electricity, because they only can retain this state of the particles (1304.).

1299. The principle of induction is of the utmost generality in electric action. It constitutes charge in every ordinary case, and probably in every case; it appears to be the cause of all excitement, and to precede every current. The degree to which the particles are affected in this their forced state, before discharge of one kind or another supervenes, appears to constitute what we call *intensity*.

1300. When a Leyden jar is *charged*, the particles of the glass are forced into this polarized and constrained condition by the electricity of the charging apparatus. *Discharge* is the return of these particles to their natural state from their state of tension, whenever the two electric forces are allowed to be disposed of in some other direction.

1301. All charge of conductors is on their surface, because being essentially inductive, it is there only that the medium capable of sustaining the necessary inductive state begins. If the conductors are hollow and contain air or any other di-

* The theory of induction which I am stating does not pretend to decide whether electricity be a fluid or fluids, or a mere power or condition of recognised matter. That is a question which I may be induced to consider in the next or following series of these researches.

electric, still no *charge* can appear upon that internal surface, because the dielectric there cannot assume the polarized state throughout, in consequence of the opposing actions in different directions.

1302. The known influence of *form* is perfectly consistent with the corpuscular view of induction set forth. An electrified cylinder is more affected by the influence of the surrounding conductors (which complete the condition of charge) at the ends than at the middle, because the ends are exposed to a greater sum of inductive forces than the middle; and a point is brought to a higher condition than a ball, because by relation to the conductors around, more inductive force terminates on its surface than on an equal surface of the ball with which it is compared. Here too, especially, can be perceived the influence of the lateral or transverse force (1297.), which, being a power of the nature of or equivalent to repulsion, causes such a disposition of the lines of inductive force in their course across the dielectric, that they must accumulate upon the point, the end of the cylinder, or any projecting part.

1303. The influence of *distance* is also in harmony with the same view. There is perhaps no distance so great that induction cannot take place through it:* but with the same constraining force (1298.) it takes place the more easily, according as the extent of dielectric through which it is exerted is lessened. And as it is assumed by the theory that the particles of the dielectric, though tending to remain in a normal state, are thrown into a forced condition during the induction; so it would seem to follow that the fewer there are of these intervening particles opposing their tendency to the assumption of the new state, the greater degree of change will they suffer, i.e. the higher will be the condition they assume, and the larger the amount of inductive action exerted through them.

1304. I have used the phrases *lines of inductive force* and *curved lines of force* (1231. 1297. 1298. 1302.) in a general sense only, just as we speak of the lines of magnetic force. The lines are imaginary, and the force in any part of them is of course the resultant of compound forces, every molecule

* I have traced it experimentally from a ball placed in the middle of the large cube formerly described (1173.) to the sides of the cube six feet distant, and also from the same ball placed in the middle of our large lecture-room to the walls of the room at twenty-six feet distance, the charge upon the ball in these cases being solely due to induction through these distances.

being related to every other molecule in *all* directions by the tension and reaction of those which are contiguous. The transverse force is merely this relation considered in a direction oblique to the lines of inductive force, and at present I mean no more than that by the phrase. With respect to the term *polarity* also, I mean at present only a disposition of force by which the same molecule acquires opposite powers on different parts. The particular way in which this disposition is made will come into consideration hereafter, and probably varies in different bodies, and so produces variety of electrical relation. All I am anxious about at present is, that a more particular meaning should not be attached to the expressions used than I contemplate. Further inquiry, I trust, will enable us by degrees to restrict the sense more and more, and so render the explanation of electrical phenomena day by day more and more definite.

1305. As a test of the probable accuracy of my views, I have throughout this experimental examination compared them with the conclusions drawn by M. Poisson from his beautiful mathematical inquiries.* I am quite unfit to form a judgment of these admirable papers; but as far as I can perceive, the theory I have set forth and the results I have obtained are not in opposition to such of those conclusions as represent the final disposition and state of the forces in the limited number of cases he has considered. His theory assumes a very different mode of action in induction to that which I have ventured to support, and would probably find its mathematical test in the endeavour to apply it to cases of induction in curved lines. To my feeling it is insufficient in its mode of accounting for the retention of electricity upon the surface of conductors by the pressure of the air, an effect which I hope to show is simple and consistent according to the present view; and it does not touch voltaic electricity, or in any way associate it and what is called ordinary electricity under one common principle.

I have also looked with some anxiety to the results which that indefatigable philosopher Harris has obtained in his investigation of the laws of induction,† knowing that they were experimental, and having a full conviction of their exactness; but I am happy in perceiving no collision at present between them and the views I have set forth.

1306. Finally, I beg to say that I put forth my particular view with doubt and fear, lest it should not bear the test of

* *Mémoires de l'Institut*, 1811, tom. xii. the first page 1, and the second paging 163.

† *Philosophical Transactions*, 1834, p. 213.

general examination, for unless true it will only embarrass the progress of electrical science. It has long been on my mind, but I hesitated to publish it until the increasing persuasion of its accordance with all known facts, and the manner in which it linked together effects apparently very different in kind, urged me to write the present paper. As I yet see no inconsistency between it and nature, but, on the contrary think I perceive much new light thrown by it on her operations; and my next papers will be devoted to a review of the phenomena of conduction, electrolyzation, current, magnetism, retention, discharge, and some other points, with an application of the theory to these effects, and an examination of it by them.

Royal Institution,
Nov. 16, 1837.

XII. *The variation of the Compass occasioned by a non-coincidence between the pole of the earth and the pole of the ecliptic, communicated to the Editor of the Annals of Electricity.* By EDWARD NAYLER, Esq., First Lieutenant of the Royal Marines.

(Continued from page 529, Vol. 3.)

From the observations of Captain Ross (first voyage) compared with the tables of variation before mentioned, it would appear that London in the year 1819 was situated on a meridian $67^{\circ} 57' 53''$ west of the meridian $a A g$, fig. 1, Pl. XVI. Vol. 3; and that the meridian $a b g$, or that on which the magnetic pole s was situated, at that time (1819) was $112^{\circ} 2' 7''$ west of London for $67^{\circ} 57' 53'' + 112^{\circ} 2' 7'' = 180^{\circ}$ or the distance between the meridians $a b g$, and $a A g$. This is proved by the variation at that period having ceased from being east, and become west; or, in other words, by the needle being attracted to the point s on the meridian $a b g$, and thence made to deviate to the westward of a the true north.

It would also appear, that, since variation in London was observed to change from east to west, the meridian on which according to our former reasoning those changes were effected ($a A g$), has passed on to the eastward of London: being now situated in longitude $67^{\circ} 57' 53''$ east. Now if we estimate the time consumed in this quantum of motion, considering the motion uniform, we shall get the period occupied in an entire revolution of the magnetic pole s round the earth. And if it should appear that this time actually constitutes an aliquot part of the period occupied in an entire revolution of the equinoctial pole, the motion of the magnetic pole, may, it is

hoped, be presumed to depend upon the motion of the equinoctial. And, if it should so appear, it is trusted an additional motive will be presented for prosecuting the enquiry in the manner begun; although, at present, no reason can be given why the needle should be attracted to a point on the arctic circle, derivable from the pole of the ecliptic. We shall proceed then to show that the motion of the magnetic pole, as to time, depends on, and is an aliquot of the motion of, the equinoctial.

It is difficult to say accurately, at what time the variation changed from east to nothing in London. If Mr. Bond's account be taken, it was in the year 1657, which period we shall at present use; reserving the right of correcting it hereafter, if it should appear to require correction; or any one more certain present itself, in the course of enquiry. Counting then from 1657 to 1819, time of Captain Ross's voyage, a period of 162 years has elapsed since the variation ceased in London. If then the motion of the magnetic pole be uniform, its revolution will be as follows:

$$\frac{162 \times 360}{67^{\circ} 57' 53''} = \overset{\text{years}}{858} \overset{\text{d.}}{17} \overset{\text{h.}}{6} \overset{\text{m.}}{5} \overset{\text{s.}}{9} \frac{132843}{244673}$$

But if the magnetic pole, according to the data given, performs a revolution round the earth in the above period, and the equinoctial pole a revolution according to some astronomers in 25920 years, the movement of the one may be considered an aliquot part of the movement of the other, for

$$\frac{25920}{858.17.6.5.9. \&c.} = 30.20811$$

Now considering the data from which this has been taken, as that on which a perfect reliance cannot be placed: that is to say, the distance of London from the meridian *a A g* shown in Captain Ross's voyage having been obtained by an approximate investigation; and that we cannot speak accurately as to the time when London was on the meridian *a A g*, we may therefore consider the decimal .20811 as what may be rejected, and the revolution of the magnetic pole be estimated in relation to that of the equinoctial as 1 to 30.

Again, if the magnetic pole performs its revolution in one thirtieth part of the time, consumed in a revolution of equinoctial, and it be admitted that La Place has given the true mean annual motion of the ecliptic, at $50''.18$ the magnetic pole performs its revolution in 860.9 years; for

$$\begin{array}{l} \overset{\text{year}}{50''.18} : 1 : : 360 : 25827.022718 \\ \text{and } \frac{25827.022718}{30} = 860.900757 \end{array}$$

giving a mean annual motion for the magnetic pole of $25^{\circ} 5' .4$ nearly.

Now, if it were possible to fix the position of the magnetic meridian abg , for any given period of time, in relation to London, or any other place, its mean annual motion would give its position for any other time or place; and thence the distance of any place from the meridian abg might be shown; and consequently quantities of variation be worked for, and thence a comparison be instituted between the quantities found in nature and those generated by our course of reasoning. Again, if quantities of variation obtained from the reasoning adduced, should be found to agree substantially with what is found in nature, so much probability of truth would be presented as ought, in this age of enquiry, to secure for the subject a patient investigation. I shall, therefore, now proceed to show, how, by the mean annual motion of the magnetic pole, the position of the meridian aAg and thence the meridian abg may be fixed, in relation to London; and having so done, institute a comparison between the quantities of variation found by Capt. Parry, in his first voyage, and those derived from our course of reasoning.

There are two modes by which our object may be attempted, one in relation to the period, when there was no variation in London, according to Mr. Bond; and another in reference to the time when Mr. Burrows, in 1530, is said to have found $11^{\circ} 15' E.$ at Limehouse. From the slow manner in which variation would alter, on its change from east to nothing, and then becoming west, it is probable much uncertainty prevailed, as to the time when there was no variation in London; that is when London was on the meridian aAg . If, however, that event took place, according to Mr. Bond in the year 1657, giving the meridians aAg and abg each a mean annual motion of $25' 5'' .4$ the meridian aAg would have passed on to the eastward of London, and been situated in the year 1819 in longitude $67^{\circ} 44' 34'' .8$ east because $1819 - 1657 = 162$. and $162 \times 25' 5'' .4 = 67^{\circ} 44' 34'' .8$. And the meridian abg have been situated in longitude $112^{\circ} 15' 25'' .2$ west for $67^{\circ} 44' 34'' .8 + 112^{\circ} 15' 25'' .2 = 180^{\circ}$ that is the distance between the meridians aAg and abg . Therefore, the magnetic pole in 1819 would have been situated in longitude $112^{\circ} 15' 25'' .2 W.$

But if the magnetic pole had been so situated in 1819, Capt. Parry in that year, in Winter Harbour, in longitude $110^{\circ} 49'$ west, would have been in westerly variation, for he would have been $1^{\circ} 26' 25'' .2$ to the eastward of the magnetic meridian abg ; inasmuch as $110^{\circ} 49' + 1^{\circ} 26' 25'' .2 = 112^{\circ} 15' 25'' .2$ and consequently he would not have found easterly variation as he did.

But taking the observation of Mr. Burrows in 1580, when he found $1^{\circ} 15'$ east, and working for the period when there would be no variation in London, we shall find it to have happened about the year 1651; and using that time (instead of Mr. Bond's) with the mean annual motion of the magnetic pole, as will be shown hereafter, it will appear that the meridian aAg in 1819 was situated in $70^{\circ} 15' 7''.2$ east longitude; for $1819 - 1651 = 168$, and $168 \times 25' 5''.4 = 70^{\circ} 15' 7''.2$. Now if the meridian aAg in the year 1819 was situated in longitude $70^{\circ} 15' 7''.2$ east, then Capt. Parry in that year in Winter Harbour, longitude $110^{\circ} 49'$ west, would have found easterly variation for $180^{\circ} - 70^{\circ} 15' 7''.2 = 109^{\circ} 44' 52''$. 8, consequently he in Winter Harbour, would have been at a distance of $1^{\circ} 4' 7''.2$ west of the magnetic meridian abg , for $109^{\circ} 44' 52''.8 + 1^{\circ} 4' 7''.2 = 110^{\circ} 49'$. Therefore, from the circumstance of Capt. Parry having found eastern variation in Winter Harbour, it has been thought best to fix the position of the magnetic pole by the observation of Mr. Burrows in 1580; and it appears to be no inconsiderable confirmation of the principles set out with in this enquiry, that the two very distant observations, as to time and place of Capt. Parry and Mr. Burrows, should be reduced by the course of reasoning adduced, to a state of accordance, after a lapse of 239 years, computing from 1580 to 1819. But taking the 71 years from 1580 under easterly variation, and half the time consumed in an entire revolution of the magnetic pole, being the time involved in the distance between the two meridians aAg and abg , and the two observations will have agreed with each other after a lapse of 501 years.

The manner in which the position of the meridian aAg from Mr. Burrows's observation has been deduced, is as follows:—Its position at the time when there was no variation in London was assumed as 0; then $25' 5''.4$ east was taken for its position in the succeeding year: adding this quantity successively every year for its change of position. With the quantities so obtained constituting the supplemental \angle of magnetic distance, mentioned in the formula before recited, (page 526, Vol. 3.) together with the co-latitude of London, and the obliquity of the ecliptic, variation was worked for, according to the formula before mentioned; which was continued until an angle of variation was obtained, equal to $11^{\circ} 14' 51''$; which differing very little from $11^{\circ} 15'$, it was placed against the year 1580. Then counting back from that year and placing the quantities of variation, previously found, against their respective years, the table was formed which is here presented, which shows the position of the meridian aAg in relation to London from the year 1466 to 1836.

Time.	West.	Time.	East.	Sup. \angle of mag- netic distance or merid. α A g.			Variation in London.		
				o	'	"	o	'	"
1651		1651							
1652	west	1650	east		25	5.4		8	35
1653	"	1649	"		50	10		19	10
1654	"	1648	"	1	15	16		28	42
1655	"	1647	"	1	40	21		38	18
1656	"	1646	"	2	5	27		47	53
1657	"	1645	"	2	30	32		57	24
1658	"	1644	"	2	55	37	1	6	58
1659	"	1643	"	3	20	43	1	16	31
1660	"	1642	"	3	45	48	1	26	14
1661	"	1641	"	4	10	54	1	35	41
1662	"	1640	"	4	35	59	1	47	19
1663	"	1639	"	5	1	4	1	54	52
1664	"	1638	"	5	26	10	2	4	25
1665	"	1637	"	5	51	15	2	13	57
1666	"	1636	"	6	16	21	2	23	30
1667	"	1635	"	6	41	26	2	33	3
1668	"	1634	"	7	6	31	2	42	35
1669	"	1633	"	7	31	37	2	52	8
1670	"	1632	"	7	56	42	3	1	41
1671	"	1631	"	8	21	48	3	11	14
1672	"	1630	"	8	46	53	3	20	46
1673	"	1629	"	9	11	58	3	30	19
1674	"	1628	"	9	37	4	3	39	58
1675	"	1627	"	10	2	9	3	49	34
1676	"	1626	"	10	27	15	3	59	7
1677	"	1625	"	10	52	20	4	8	40
1678	"	1624	"	11	17	25	4	18	2
1679	"	1623	"	11	42	31	4	27	35
1680	"	1622	"	12	7	36	4	37	8
1681	"	1621	"	12	32	42	4	46	41
1682	"	1620	"	12	57	47	4	56	13
1683	"	1619	"	13	22	52	5	5	46
1684	"	1618	"	13	47	58	5	15	19
1685	"	1617	"	14	13	3	5	24	51
1686	"	1616	"	14	38	9	5	34	24
1687	"	1615	"	15	3	14	5	43	57
1688	"	1614	"	15	28	19	5	53	29
1689	"	1613	"	15	53	25	6	3	2
1690	"	1612	"	16	18	30	6	12	25
1691	"	1611	"	16	43	36	6	21	58
1692	"	1610	"	17	8	41	6	31	30
1693	"	1609	"	17	33	46	6	41	3
1694	"	1608	"	17	58	52	6	50	26
1695	"	1607	"	18	23	57	6	59	58

Time.	West.	Time.	East.	Sup. \angle of mag- netic distance or merid. α Δ g.			Variation in London.		
				o	'	"	o	'	"
1696	west	1606	east	18	49	3	7	9	31
1697	"	1605	"	19	14	8	7	18	54
1698	"	1604	"	19	39	13	7	28	26
1699	"	1603	"	20	4	19	7	37	59
1700	"	1602	"	20	29	24	7	47	22
1701	"	1601	"	20	54	30	7	56	55
1702	"	1600	"	21	19	35	8	6	27
1703	"	1599	"	21	44	40	8	15	50
1704	"	1598	"	22	9	46	8	25	13
1705	"	1597	"	22	34	51	8	34	45
1706	"	1596	"	22	59	57	8	44	8
1707	"	1595	"	23	25	2	8	53	41
1708	"	1594	"	23	50	7	9	3	7
1709	"	1593	"	24	15	13	9	12	36
1710	"	1592	"	24	40	18	9	21	59
1711	"	1591	"	25	5	24	9	31	22
1712	"	1590	"	25	30	29	9	40	54
1713	"	1589	"	25	55	34	9	50	17
1714	"	1588	"	26	20	40	9	59	40
1715	"	1587	"	26	45	45	10	9	12
1716	"	1586	"	27	10	51	10	18	35
1717	"	1585	"	27	35	56	10	27	58
1718	"	1584	"	28	1	1	10	37	20
1719	"	1583	"	28	26	7	10	46	53
1720	"	1582	"	28	51	12	10	56	6
1721	"	1581	"	29	16	18	11	5	29
1722	"	1580	"	29	41	23	11	14	51
1723	"	1579	"	30	6	28	11	24	14
1724	"	1578	"	30	31	34	11	33	37
1725	"	1577	"	30	56	39	11	42	59
1726	"	1576	"	31	21	45	11	52	22
1727	"	1575	"	31	46	50	12	1	45
1728	"	1574	"	32	11	55	12	10	57
1729	"	1573	"	32	37	1	12	20	20
1730	"	1572	"	33	2	6	12	29	43
1731	"	1571	"	33	27	12	12	38	56
1732	"	1570	"	33	52	17	12	48	18
1733	"	1569	"	34	17	22	12	57	31
1734	"	1568	"	34	42	28	13	7	14
1735	"	1567	"	35	7	33	13	16	16
1736	"	1566	"	35	32	39	13	25	29
1737	"	1565	"	35	57	44	13	34	42
1738	"	1564	"	36	22	49	13	44	4
1739	"	1563	"	36	47	55	13	53	17
1740	"	1562	"	37	13	"	14	2	30

Time.	West.	Time.	East.	Sup. \angle of mag- netic distance or merid. a A g .			Variation in London.		
				o	'	"	o	'	"
1741	west	1561	east	37	38	6	14	11	53
1742	"	1560	"	38	3	11	14	21	5
1743	"	1559	"	38	28	16	14	30	18
1744	"	1558	"	38	53	22	14	39	31
1745	"	1557	"	39	18	27	14	48	43
1746	"	1556	"	39	43	33	14	57	56
1747	"	1555	"	40	8	38	15	7	9
1748	"	1554	"	40	33	43	15	16	21
1749	"	1553	"	40	58	49	15	25	34
1750	"	1552	"	41	23	54	15	34	37
1751	"	1551	"	41	49	"	15	43	30
1752	"	1550	"	42	14	5	15	53	2
1753	"	1549	"	42	39	10	16	2	5
1754	"	1548	"	43	4	16	16	11	18
1755	"	1547	"	43	29	21	16	20	30
1756	"	1546	"	43	54	27	16	29	33
1757	"	1545	"	44	19	32	16	38	36
1758	"	1544	"	44	44	37	16	47	48
1759	"	1543	"	45	9	43	16	56	51
1760	"	1542	"	45	34	48	17	6	4
1761	"	1541	"	45	59	54	17	14	57
1762	"	1540	"	46	24	59	17	24	9
1763	"	1539	"	46	50	4	17	33	12
1764	"	1538	"	47	15	10	17	42	15
1765	"	1537	"	47	40	15	17	51	7
1766	"	1636	"	48	5	21	18	"	20
1767	"	1535	"	48	30	26	18	9	23
1768	"	1534	"	48	55	31	18	18	15
1769	"	1533	"	49	20	37	18	27	18
1770	"	1532	"	49	45	42	18	36	11
1771	"	1531	"	50	10	48	18	45	14
1772	"	1530	"	50	35	53	18	54	6
1773	"	1529	"	51	"	58	19	3	9
1774	"	1528	"	51	26	4	19	12	2
1775	"	1527	"	51	51	9	19	20	54
1776	"	1526	"	52	16	15	19	30	2
1777	"	1525	"	52	41	20	19	38	50
1778	"	1524	"	53	6	25	19	47	42
1779	"	1523	"	53	31	31	19	56	35
1780	"	1522	"	53	56	36	20	5	18
1781	"	1521	"	54	21	42	20	14	11
1782	"	1520	"	54	46	47	20	23	3
1783	"	1519	"	55	11	52	20	31	56
1784	"	1518	"	55	36	58	20	40	39
1785	"	1517	"	56	2	3	20	49	31

Time.	West.	Time.	East.	Sup. \angle of mag- netic distance or merid. α A g.			Variation in London.		
				o	'	"	o	'	"
1786	west	1516	east	56	27	9	20	58	14
1787	"	1515	"	56	52	14	21	6	57
1788	"	1514	"	57	17	19	21	15	29
1789	"	1513	"	57	42	25	21	24	42
1790	"	1512	"	58	7	30	21	33	15
1791	"	1511	"	58	32	36	21	41	58
1792	"	1510	"	58	57	41	21	50	40
1793	"	1509	"	59	22	46	21	59	23
1794	"	1508	"	59	47	52	22	8	56
1795	"	1507	"	60	12	57	22	16	48
1796	"	1506	"	60	38	3	22	25	11
1797	"	1505	"	61	3	8	22	33	54
1798	"	1504	"	61	28	13	22	42	26
1799	"	1503	"	61	53	19	22	51	9
1800	"	1502	"	62	18	24	22	59	42
1801	"	1501	"	62	43	30	23	8	15
1802	"	1500	"	63	8	35	23	16	47
1803	"	1499	"	63	33	40	23	25	20
1804	"	1498	"	63	58	46	23	33	50
1805	"	1497	"	64	23	51	23	42	15
1806	"	1496	"	64	48	57	23	50	38
1807	"	1495	"	65	14	2	23	59	11
1808	"	1494	"	65	39	7	24	7	43
1809	"	1493	"	66	4	13	24	16	6
1810	"	1492	"	66	29	18	24	24	29
1811	"	1491	"	66	54	24	24	32	42
1812	"	1490	"	67	19	29	24	41	14
1813	"	1489	"	67	44	34	24	49	27
1814	"	1488	"	68	9	40	24	57	50
1815	"	1487	"	68	34	45	25	6	12
1816	"	1486	"	68	59	51	25	14	35
1817	"	1485	"	69	24	56	25	22	48
1818	"	1484	"	69	50	1	25	31	"
1819	"	1483	"	70	15	7	25	39	13
1820	"	1482	"	70	40	12	25	47	26
1821	"	1481	"	71	5	18	25	55	39
1822	"	1480	"	71	30	23	26	3	51
1823	"	1479	"	71	55	28	26	11	54
1824	"	1478	"	72	20	34	26	19	57
1825	"	1477	"	72	45	39	26	28	9
1826	"	1476	"	73	10	45	26	36	12
1827	"	1475	"	73	35	50	26	44	25
1828	"	1474	"	74	"	55	26	52	22
1829	"	1473	"	74	26	1	27	"	20
1830	"	1472	"	74	51	6	27	8	23

Time.	West.	Time.	East.	Sup. \angle of mag- netic distance or merid $a A g$.			Variation in London.		
				°	'	"	°	'	"
1831	west	1471	east	75	16	12	27	16	16
1832	"	1470	"	75	41	17	27	24	8
1833	"	1469	"	76	6	22	27	32	1
1834	"	1468	"	76	31	28	27	40	4
1835	"	1467	"	76	56	33	27	47	41
1836	"	1466	"	77	21	39	27	55	44

We have now to examine the quantities of variation observed by Capt. Parry, during his voyage in the year 1819; and compare them with the quantities presented, according to our former reasoning, from the position of the meridian $a A g$ as shown in the preceding table. The supplemental angle of magnetic distance, or the position of the meridian $a A g$ in relation to London in the year 1819 was $70^{\circ} 15' 7''$ east of London; consequently the position of the meridian $a b g$ in reference to London was $109^{\circ} 44' 53''$ west of London, on which meridian in the year 1819 the northern magnetic pole was seated: for $109^{\circ} 44' 53'' + 70^{\circ} 15' 7'' = 180^{\circ}$ or the distance between the meridians $a b g$, and $a A g$. Therefore, $70^{\circ} 15' 7''$ was the supplemental angle of the magnetic distance of London; and as all Capt. Parry's observations were made in west longitude, the longitudes of his places of observation have been added respectively to $70^{\circ} 15' 7''$ for the supplemental angle of the magnetic distance of the place of observation. These with the co-latitude and the obliquity of the ecliptic, according to the formula before mentioned, have given the quantities of variation, set down in the table of Capt. Parry's observations; to which a correction for the local attraction of the ship has been applied.

The correction for local attraction was thus obtained. Capt. Parry, in the commencement of his voyage, made an experiment on the local attraction of his ship, at Northfleet; and one subsequently in Baffin's Bay. Comparing these two experiments, it will be seen that the local attraction of the ship augmented very considerably in her voyage to Baffin's Bay; where a high degree of natural variation existed. At Northfleet, when the ship's head was east, he discovered $4^{\circ} 41'$ less than the true bearing of an object, the natural variation of the compass being at that place $25^{\circ} 39' 13''$ west. And at Baffin's Bay with her head on the same point he discovers $15^{\circ} 5'$, less than the true bearing of an object; the natural variation of the compass in Baffin's Bay being 82° west. Now as there can be

no doubt but this effect of local attraction altered progressively throughout the voyage, and knowing nothing so likely to influence it as the natural variation of the needle, or what is the same, thinking it would be generated in a like proportion, with the natural variation of the compass, I have endeavoured to assign its quantity at every place where Capt. Parry observed the variation of the compass, by supposing that the local attraction altered, as the natural variation; thus:—

The variation of Northfleet

Is to the local attraction on any given
point of the compass,

As the variation of any other place

Is to the effect of local attraction at
that place on the given point.

By the above proportion, quantities of local attraction were obtained, which have been added to, or subtracted from, the variation worked for, to correct for the position of the ship's head, at the time of observation.

The following table shows the quantity of local attraction found by Capt. Parry, in his ship at Northfleet and Baffin's Bay. The column A was obtained by comparing the two observations by the proportion just mentioned. The two experiments in Baffin's Bay and Northfleet do not entirely agree on certain points, either from the difficulty of getting the ship's head steady on any given point, or from something in the law of local attraction with which we are not yet acquainted. But the two observations, it is hoped, do sufficiently agree, for the use which has been made of them, and show, it is trusted, a remarkable coincidence between the quantities of variation found by Capt. Parry, and those derived from our course of reasoning.

Position of the Ship's Head.	Found by experiment at Northfleet.				What should have been found in Baffin's Bay if proportionate to that found at Northfleet.				Found by Experiment in Baffin's Bay.			
		°	'	"		°	'	"		°	'	"
N	+	"	15		+	"	47	57	+	"	46	"
N δ E	—	1	"		—	3	11	47	—	3	37	15
NNE	—	2	"		—	6	23	34	—	6	28	30
NE δ N	—	2	40		—	8	31	26	—	8	40	15
NE	—	4	15		—	13	35	5	—	10	52	"
NE δ E	—	4	5		—	13	3	8	—	13	22	30
ENE	—	4	"		—	12	47	8	—	15	53	"
E δ N	—	4	41		—	14	58	12	—	15	29	"
E	—	4	41		—	14	58	12	—	15	5	"
E δ S	—	3	45		—	11	59	12	—	14	9	"
ESE	—	3	15		—	10	23	18	—	13	13	"
SE δ E	—	2	45		—	8	47	25	—	11	3	15
SE	—	1	30		—	4	47	41	—	8	53	30
SE δ S	—	1	15		—	3	59	44	—	7	31	45
SSE	—	"	15		—	"	47	57	—	6	10	"
S δ E	+	"	30		+	1	35	54	+	3	39	15
S	+	"	30		+	1	35	54	+	1	8	45
S δ W	+	1	"		+	3	11	47	+	"	9	15
SSW	+	1	"		+	3	11	47	+	"	50	"
SW δ S	+	1	30		+	4	47	41	+	3	17	"
SW	+	1	45		+	5	35	38	+	5	44	"
SW δ W	+	3	"		+	9	35	21	+	8	42	"
WSW	+	"	"		+	9	23	28	+	11	40	"
W δ S	+	"	"		+	11	37	34	+	12	49	"
W	+	"	"		+	13	51	43	+	13	58	"
W δ N	+	4	15		+	13	35	5	+	13	4	"
WNW	+	3	30		+	11	11	15	+	12	13	"
NW δ W	+	3	"		+	9	35	21	+	10	42	30
NW	+	2	45		+	8	47	25	+	9	12	"
NW δ N	+	2	25		+	7	43	29	+	7	19	"
NNW	+	2	15		+	7	11	31	+	5	26	"
N δ W	+	2	45		+	8	47	25	+	2	20	"

We have now to compare quantities of variation found by Capt. Parry, in his first voyage, with those which may be computed, if the needle be attracted to a point in the arctic circle, derived from the non-coincidence of the pole of the earth, and the pole of the ecliptic, according to the reasoning before adduced. And as in the quantities computed we have made use of the mean annual motion of the magnetic pole, so all Capt. Parry's quantities may be said to be compared with one single observation; namely that of Mr. Burrows at Limehouse, in the year 1580. We have therefore placed against each of Capt. Parry's observations, the time elapsed between his observation and that of Mr. Burrows.

Lat. North.	Long. West.	Position of Ship's Head.	Variation observed West.	Computed var. corrected for local attraction by Baffin's bay experiment.	Computed var. corrected for local attraction by Northfleet experiment.	Time elapsed. years.
57 9 1 2		N $\frac{1}{2}$ E }	25 42 " 26 27 " 27 45 "	28 16 33 28 15 21 28 13 15	28 35 16 28 34 36 28 33 28	241.47
58 24 1 25		N N E }	27 2 " 27 15 "	27 43 34 27 42 33	27 45 12 27 44 11	242.38
59 26 4 50		W N W }	36 19 " 35 56 " 36 39 " 36 22 " 36 33 "	37 17 40 37 14 15 37 20 39 37 18 7 37 19 45	36 50 19 36 47 11 36 53 3 36 50 44 36 52 14	250.55
59 " 7 26		W N W }	37 32 " 37 55 " 38 31 " 38 37 " 38 38 "	38 11 47 38 15 13 38 20 36 38 21 29 38 21 38	37 43 32 37 46 40 37 51 35 37 52 24 37 52 32	256.77
57 42 14 16		W N W }	40 23 " 40 37 " 40 39 " 41 32 " 40 46 "	40 13 31 40 16 36 40 16 54 40 24 48 40 17 57	39 44 7 39 46 1 39 46 18 39 53 31 39 47 15	273.11
57 17 16 30		W N W }	42 31 " 43 11 " 42 50 " 43 27 "	41 2 19 41 8 17 41 5 9 41 10 36	40 30 19 40 35 46 40 32 54 40 37 57	278.45
56 52 23 40		N W N W & N }	45 12 " 43 20 " 43 46 "	41 52 40 39 47 40 42 7	44 38 31 40 52 44 40 55 11	295.59
57 22 25 12		N N δ E } N N E }	41 20 " 39 20 " 39 49 " 39 29 "	38 6 35 59 40 35 58 23 34 36 48	38 8 2 36 11 52 36 10 4 34 39 10	299.26
58 14 30 27		N W $\frac{1}{2}$ W	51 12 "	46 30	8 46 1 30	"

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Lat. North.	Long. West.	Position of Ship's Head.	Variation observed West.	Computed var. correct- ed for local attraction by Baffin's bay experiment	Computed var. correct- ed for local attraction by Northfleet experiment.	Time elapsed.
58 14 30 27		N W $\frac{1}{2}$ W	51 12 "	46 30 8	46 13 0	years. 311.81
55 1 35 27		N E	38 26 " 39 9 " 37 45 " 37 37 "	38 32 " 38 32 "		323.77

The above observations appear to have been corrected for local attraction.

55	3 36	"	N W $\frac{1}{2}$ N	41 27 " 43 43 " 42 46 " 42 9 "	42 26 " 42 38 13 41 34 12 41 31 44	54 23 27 42 51 16 42 29 14 42 25 59	325.09
55	48 37 15		N N W				
			S W $\frac{1}{2}$ S	46 37 " 47 17 " 48 8 " 50 20 "	42 21 10 43 5 45 46 38 17 48 21 46	42 44 33 43 " 55 45 18 8 48 17 55	328.08
			S W				
			W S W				
			W				
55	42 37 53		W $\frac{1}{2}$ N	51 44 " 51 18 " 52 14 " 51 10 " 50 53 " 51 45 "	48 5 23 48 1 15 48 10 11 47 59 59 47 57 15 48 5 33	48 24 53 48 20 35 48 29 52 47 19 16 48 16 26 48 25 3	329.59
56	8 38	"	N	45 " " 44 56 " 44 23 " 44 29 " 45 20 "	40 44 24 40 44 24 40 44 6 40 44 9 40 44 38	40 45 31 40 45 28 40 45 9 40 45 13 40 45 42	329.83
55	46 38 9		N N E	38 44 " 39 22 " 38 57 " 40 58 " 40 18 "	39 58 49 39 58 49		330.19

Great difference is here perceived in the quantities obtained by the different observers; and they appear to have been corrected for local attraction.

Lat. North.	Long. West	Position of Ship's Head.	Variation observed West.	Computed var. corrected for local attraction by Baffin's Bay experiment	Computed var. corrected for local attraction by Northfleet experiment	Time elapsed. years.
56 9 40	"	N	43 2 "	41 14 29	41 15 30	334.65
56 37 40	22	N b W	47 4 "	42 46 34	46 28 57	335.53

Considerable difference is seen in the effect of local attraction on N b W point, in the two experiments at Northfleet and Baffin's Bay. At the former place $2^{\circ} 45'$ is discovered, and at the latter place only $2^{\circ} 20'$; when, if the latter had been in proportion to the former, there ought to have been $8^{\circ} 47' 25''$. Again, we find the difference between the variation observed and that given by theory, to point at this discrepancy, and to agree nearly with the proportion of local attraction derived from the experiment at Northfleet.

56 39 40	22	N b W $\frac{1}{2}$ W	46 40 "	43 40 59	46 1 15	335.53
			47 49 "	43 44 15	46 7 59	
			47 26 "	43 43 10	46 5 44	

See former note.

57 42 41	30	N W b W $\frac{1}{2}$ W	53 35 "	50 38 36	49 29 35	338.24
		N W b W	52 1 "	49 45 32	49 2 56	
		N W $\frac{1}{2}$ W	50 37 "	50 29 27	48 30 55	
		N W	50 7 "	48 35 19	48 20 17	
57 55 40	57	N	45 44 "	43 30 54	43 31 10	336.92
			46 50 "	43 30 42	43 31 49	
			46 41 "	43 30 37	43 31 44	
			46 43 "	43 30 38	43 31 45	
			38 8 "	36 33	36 6 44	
		E	39 20 "	35 50 20	35 53 36	
			40 25 "	35 38 22	35 41 41	
			39 30 "	35 48 29	35 51 46	
57 55 40	57	S	45 42 "	43 42 45	43 57 53	336.92
			46 47 "	43 43 39	43 59 9	
			47 5 "	43 43 54	43 59 30	
			47 8 "	43 43 57	43 59 33	
			52 4 "	50 28 54	49 2 13	
		W S W	52 23 "	50 31 36	49 4 24	
			52 42 "	50 34 19	49 6 34	
			51 59 "	50 28 12	49 1 39	

Lat. North.	Long. West.	Position of Ship's Head.	Variation observed West.	Computed var. correct- ed for local attraction by Baffin's Bay experiment.	Computed var. correct- ed for local attraction by Northfleet experiment.	Time elapsed.
° ' "	° ' "		° ' "	° ' "	° ' "	years.
57	30	42	12	W N W	54 47 " 51 42 50 23 12	339.91
				54 37 " 51 25 50 21 51		
				53 51 " 50 56 7 50 15 34		
				54 45 " 51 4 9 50 22 56		
58	11	48	30	N W $\frac{1}{2}$ W	55 43 " 52 25 51 31 48	354.97
				55 22 " 52 " 25 51 29 27		
				55 31 " 52 13 51 30 27		
				54 15 " 51 33 50 55 30		
58	19	48	29	N E δ N	45 59 " 45 30 11 45 30 11	354.94
58	29	48	25	N E $\frac{3}{4}$ N	45 43 " 45 43 " 45 43 "	354.78
58	42	48	21	N E $\frac{3}{4}$ N	45 53 " 46 " 3 46 "	354.62
58	43	48	21	N E $\frac{1}{2}$ N	45 27 " 46 1 29 46 1 29	354.62
58	39	48	21	N E $\frac{3}{4}$ N	45 23 " 45 55 53 45 55 53	354.62

These observations appear to have been corrected for local attraction.

				N δ W $\frac{1}{2}$ W	59 21 " 54 46 35 7 44 25	years.
61	24	54	45	N δ W $\frac{1}{2}$ W	57 40 " 54 8 32 57 51 26	369.92
				N δ W	58 38 " 53 37 29 58 14 30	
					59 3 " 53 38 12 58 17 11	

We have before observed upon the difference between the two experiments on local attraction, at Northfleet and Baffin's Bay, when the ship's head was N δ W; and may here remark the same coincidence noted before.

				N	49 13 " 48 " 30 48 1 40	years.
59	49	48	9		49 14 " 48 " 30 48 1 40	354.14
				N δ W	62 36 " 58 23 36 63 19 21	
					61 19 " 57 11 52 59 35 59	386.42
62	45	61	39	N $\frac{1}{2}$ W	61 20 " 57 11 52 59 36 2	
					60 57 " 57 11 39 59 34 55	
					62 7 " 57 12 19 59 38 20	

See former note on N δ W.

Lat. North.	Long. West.	Position of Ship's Head.	Variation observed West.	Computed var. corrected for local attraction by Baffin's bay experiment	Computed var. corrected for local attraction by Northfleet experiment.	Time elapsed. years.
62 50	61 40	N N E } N N E $\frac{1}{2}$ E }	49 49 " 50 17 " 50 29 " 50 30 "	52 52 17 6 " 52 50 4 52 53 5 " 52 49 8 52 1 59 " 52 6 27 52 12 43 "	55 16 53 5 1 59 12 43	386.46
63 30	61 43	E N E	45 44 "	49 29 19 51 12 59		386.58
64 "	61 50	N 10° E } N δ E $\frac{1}{2}$ E }	58 48 " 54 29 " 55 43 "	57 20 8 57 44 40 " 56 21 51 56 31 57 " 56 17 18 56 27 38 "		336.86
63 40	62 10	N 59° E	49 11 "	52 45 52 51 5 47		387.66
62 26	62 28	N 86° E }	47 52 " 47 44 " 48 25 " 48 20 "	50 26 12 50 35 9 " 50 27 41 50 36 37 " 50 20 4 50 29 8 " 50 21 " 50 30 2 "		388.38
64 1	60 55	E N E }	49 1 " 49 28 "	49 49 52 51 40 58 " 49 34 28 51 36 45 "		384.66
64 "	60 55	N 81° E	48 49 "	50 4 37 50 20 6		384.66
64 2	59 45	E }	50 28 " 50 51 "	49 37 24 49 41 35 " 49 33 10 49 37 23 "		381.88
64 4	60 50	E $\frac{1}{2}$ S	51 11 "	50 15 6 50 57 44		384.47
66 50	57 12	E δ S	51 59 "	55 29 " 56 51 47		375.78
67 49	57 55	E N E }	57 8 " 60 9 "	56 17 5 58 26 41 " 55 42 1 57 58 22 "		377.5

There is much difference in the quantities observed.

67 42	57 35	N δ W $\frac{1}{2}$ W }	71 30 " 69 21 " 70 2 " 72 43 " 69 43 "	70 14 40 73 49 35 " 68 49 55 74 17 34 " 68 51 5 74 21 58 " 67 23 30 72 31 40 " 67 31 29 70 15 20 "	376.7
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There are great differences between the two experiments on local attraction on all these bearings; see former note on N δ W.

Lat. North.	Long. West.	Position of Ship's Head.	Variation observed West.			Computed var. corrected for local attraction by Baffin's bay experiment.			Computed var. corrected for local attraction by Northfleet experiment.			Time elapsed.
												years.
67 52	57 56	E	60	42	"	56	19	35	56	24	37	377.54
68 21	57 2	NE δ N {	60	11	"	61	51	53	61	58	21	375.38
		60 19 {	60	19	"	61	51	2	61	57	31	
		N 70° E {	55	44	"	57	29	37	59	12	30	
			55	58	"	57	26	55	59	10	14	
68 29	57 6	N 19° E	67	13	"	64	22	1	64	11	38	375.15
68 28	57 17	N δ E {	64	19	"	65	50	47	66	10	45	375.98
		66 1 {	66	1	"	65	46	17	66	6	47	
		N δ E $\frac{1}{2}$ E	64	51	"	65	15	30	65	31	35	
69 3	57 58	N {	67	23	"	70	3	17	70	1	41	377.62
			68	16	"	70	"	47	70	1	41	
70 43	59 25	NE }	69	13	"	67	17	40	65	"	"	381.08
			67	34	"	67	80	47	65	16	24	
			69	33	"	67	15	"	64	56	40	
72 56	58 37	N δ E	72	32	"	79	19	21	79	41	52	379.17
		N N E {	70	56	"	76	55	27	76	59	43	
			71	15	"	76	53	57	76	58	14	
73 13	60 15	N 67° W	99	42	"	99	40	2	98	23	30	383.08
		N 28° W {	92	4	"	92	31	11	93	50	28	
			90	33	"	91	53	31	93	6	17	
74 1	75 "	N 5° E	94	32	"	101	17	12	102	30	22	418.35
		NNE $\frac{1}{2}$ E	86	47	"	95	33	33	95	44	19	
73 51	89 7	N 15° E	112	23	"	118	33	55	118	58	46	452.11
73 53	75 70	N δ W }	104	48	"	104	8	7	104	8	7	420.34
			108	12	"							
			106	3	"							
			105	4	"							

Great difference may be perceived here in the quantities of variation observed; and the quantities appear to have been corrected for local attraction.

Lat. North.		Long. West.		Position of Ship's Head.	Variation observed West.	Computed var. corrected for local attraction by Baffin's bay experiment	Computed var. corrected for local attraction by Northfleet experiment.	Time elapsed.
					° ' "	° ' "	° ' "	years.
72	54	90	22	N 20° W	137.12	130.32 10	135.13 33	455.1
				N 18° W	132.24	129.22 7	135.14 33	
				N 15° W	132.58	127.59 26	135.59 16	

We have before remarked on the difference between the two experiments on local attraction, at Northfleet and Baffin's Bay, when the ship's head was N 6° W; and may here notice the same coincidence in relation to the variation observed and that computed, which nearly agrees with the experiment at Northfleet.

In the comparison which has been instituted between Capt. Parry's observations and the quantities computed under our former reasoning, an entire coincidence could not have been expected; various reasons may be given why it should not have been hoped for.

First, we are not acquainted with the manner in which local attraction altered during the progress of the voyage. It may have changed somewhat, after the proportion supposed, influenced by the natural variation of the compass.

Secondly, the discrepancies between the two observations at Northfleet and Baffin's Bay show that the mode of estimating the local attraction of a ship is attended with difficulties which require a remedy, before quantities obtained as they now are can be implicitly relied on; so as to be made available to the purposes to which we have endeavoured to apply them.

Thirdly, the compass at present is not a perfect instrument; this is shown by different quantities of variation being obtained by different persons at the same time and place.

Fourthly, a more certain knowledge must be obtained as to the time when there was no variation on the meridian of London; any error in this particular would occasion the variation computed to be erroneous.

However, acknowledging the existence of obstacles to a perfect coincidence, it is hoped the comparison instituted between Captain Parry's quantities of variation and those derived from our former reasoning, will be considered highly favorable. It does not seem possible that so near an approach, uniformly could ever have been made to the quantities observed by Captain Parry, if there was no truth in the reasoning by which the computed quantities were derived. The

question, therefore, which I would with much earnestness press upon the consideration of the honourable Board, is, whether under the weight of all the obstacles enumerated, there has not been a sufficient coincidence shown between Captain Parry's quantities of variation and those derived from the reasoning we have adduced, to warrant a further investigation ; which might be carried on (without expense to the government, or inconvenience to its officers,) with every probability that this most recondite subject, which has eluded human investigation since its first discovery by Columbus, should be carried ultimately, and in a short time, to an entire and perfect state of development.

An extensive and comprehensive enquiry might be instituted as follows :—His Majesty's ships should be directed to make observations daily on the variation of the compass, in the manner in which Captain Parry made his observations ; that is noting down the position of the ship's head at the time of observation. The local attraction of the ship should also be ascertained before she proceeded to sea ; and, subsequently, at every port she visited, that its various changes might be recorded. These observations on variation, and local attraction, transmitted to the hydrographer's office would soon furnish materials for proving or disproving what has been adduced. These observations would tend, in the first place, to show the laws of local attraction ; that is, whether they alter in the manner and ratio in which the variation of the compass alters, or after the order of any peculiar law of their own, influenced by the materials of which a ship is constructed. That local attraction is modified by that which influences the variation of the compass, is extremely probable from a circumstance which has not yet been noticed, namely, a probability that the south pole of the earth maintains an influence in the northern hemisphere, and the north pole an influence in the southern hemisphere. The possibility of this is shown by the reasoning on variation which we have adopted, although the circumstance has not yet occasioned any remark ; it having been thought better to reserve its consideration until the present moment.

At page 524, Vol. 3, we have said " In all places between the meridians $b D g$ and $C o g$, there will be easterly variation, because the magnetic pole o is to the right of the true pole g ." But we would now further observe, that along the equator $b d c$, fig. 1, Pl. XVI. there would be a double quantity of variation with opposite characters ; for s is the north magnetic pole, and o is the south magnetic pole. Now let d be any place on the equator ; then $b g$ expresses the distance of the meridian

adg from the meridian abg or northern magnetic meridian; again dc expresses the distance of the meridian adg from the meridian acg or southern magnetic meridian. But bd and dc are respectively supplements to each other, and being so can only agree and be equal to each other, when respectively equal to 90° ; in which case variation in relation to either hemisphere would agree in quantity, but be opposite in character; that is, if in the northern hemisphere, it was west, in the southern it would be east, and on the contrary. Now it cannot be possible that variation on the equator should have a double character; it cannot be at the same time both east and west, without the quantities in some way or other modifying and influencing each other, so as to produce some fixed and specific character.

But if two distinct quantities of variation cannot subsist on the equator without influencing each other, so as to produce one specific quantity of a determinate character; so neither is it probable that opposite characters in variation can subsist in opposite hemispheres without influencing each other so as to make some correction necessary, when in either hemisphere quantities of variation are computed according to the reasoning which has been adduced. Now this correction might result in observing the manner in which local attraction would change as a ship proceeded to different parts of the world, and thence through different quantities of variation influence; for it is extremely probable that this influence acts on the materials of which a ship is constructed, so as to modify and alter their line of influence; and hence it is conceived has arisen the inutility of any correcting plate for the compass, fixed, according to the local attraction of a ship, at some particular spot: there being some disturbing force, variable both as to position and intensity, modifying from time to time the local attraction of a ship. As that which may illustrate what has been said, we shall now show the double character in which variation would appear on the equator, and afterwards make a few remarks.

VARIATION ON THE EQUATOR.

Angle <i>b a d</i> Fig. 1.	Angle <i>d a c</i> Fig. 1.	Variation East.		Variation West.		Difference.		Sign.
1	179	13	25	24	14	10	49	west
2	178	26	48	48	25	21	37	"
3	177	40	12	12	35	32	23	"
4	176	53	36	36	42	43	6	"
5	175	1 6	58	2 "	43	53	45	"
6	174	1 20	20	2 24	40	1 4	20	"
7	173	1 33	43	2 48	29	1 14	46	"
8	172	1 47	4	3 12	11	1 25	7	"
9	171	2 "	24	3 35	45	1 35	21	"
10	170	2 13	43	3 59	8	1 45	25	"
11	169	2 27	2	4 22	20	1 55	18	"
12	168	2 40	19	4 45	22	2 5	3	"
13	167	2 53	35	5 8	10	2 14	35	"
14	166	3 6	49	5 30	40	2 23	51	"
15	165	3 20	2	5 53	4	2 33	2	"
16	164	3 33	13	6 15	8	2 41	55	"
17	163	3 46	23	6 36	57	2 50	34	"
18	162	3 59	31	6 58	27	2 58	55	"
19	161	4 12	36	7 19	41	3 7	5	"
20	160	4 25	40	7 40	36	3 14	56	"
21	159	4 38	44	8 1	12	3 22	28	"
22	158	4 51	41	8 21	26	3 29	45	"
23	157	5 4	19	8 41	20	3 37	1	"
24	156	5 17	33	9 "	55	3 43	22	"
25	155	5 30	25	9 20	7	3 49	42	"
26	154	5 43	14	9 38	57	3 55	43	"
27	153	5 56	"	9 57	24	4 1	24	"
28	152	6 8	44	10 15	28	4 6	44	"
29	151	6 21	25	10 33	8	4 11	43	"
30	150	6 34	2	10 50	25	4 16	23	"
31	149	6 46	36	11 8	14	4 21	38	"
32	148	6 59	6	11 23	45	4 24	39	"
33	147	7 11	33	11 39	48	4 28	15	"
34	146	7 23	48	11 55	25	4 31	37	"
35	145	7 36	16	12 10	38	4 34	22	"
36	144	7 48	31	12 25	25	4 36	54	"
37	143	8 "	42	12 39	47	4 39	5	"
38	142	8 12	49	12 53	43	4 40	54	"
39	141	8 25	2	13 7	13	4 42	11	"
40	140	8 37	1	13 20	17	4 43	16	"
41	139	8 48	45	13 32	56	4 44	11	"
42	138	9 "	34	13 45	8	4 44	34	"
43	137	9 12	18	13 56	55	4 44	37	"
44	136	9 24	7	14 8	16	4 44	9	"
45	135	9 35	51	14 19	12	4 43	41	"

VARIATION ON THE EQUATOR.

Angle <i>b a d</i> Fig. 1.	Angle <i>d a c</i> Fig. 1.	Variation East.			Variation West.			Difference.			Sign.
•	•	•	•	•	•	•	•	•	•	•	•
46	134	9	47	“	14	29	42	4	42	42	west
47	133	9	58	53	14	39	45	4	40	52	“
48	132	10	9	41	14	49	24	4	39	43	“
49	131	10	21	3	14	58	38	4	37	35	“
50	130	10	32	9	15	7	26	4	35	17	“
51	129	10	43	9	15	16	“	4	32	51	“
52	128	10	54	3	15	23	48	4	29	45	“
53	127	11	4	40	15	31	22	4	26	42	“
54	126	11	16	20	15	38	31	4	22	11	“
55	125	11	26	7	15	45	17	4	19	10	“
56	124	11	36	24	15	51	38	4	15	14	“
57	123	11	46	45	15	57	36	4	10	51	“
58	122	11	57	8	16	3	10	4	6	2	“
59	121	12	7	5	16	8	21	4	1	16	“
60	120	12	17	4	16	13	9	3	56	5	“
61	119	12	27	5	16	17	34	3	50	29	“
62	118	12	36	38	16	21	37	3	44	59	“
63	117	12	46	13	16	25	18	3	39	5	“
64	116	12	56	40	16	28	36	3	31	56	“
65	115	13	5	“	16	31	34	3	26	34	“
66	114	13	14	10	16	34	9	3	19	59	“
67	113	13	23	12	16	36	25	3	13	13	“
68	112	13	32	4	16	38	19	3	6	15	“
69	111	13	40	48	16	39	52	2	59	4	“
70	110	13	49	22	16	41	6	2	51	44	“
71	109	13	57	47	16	42	“	2	44	13	“
72	108	14	6	2	16	42	34	2	36	32	“
73	107	14	14	7	16	42	49	2	28	42	“
74	106	14	22	3	16	42	45	2	20	42	“
75	105	14	29	46	16	42	22	2	12	36	“
76	104	14	37	20	16	41	40	2	4	20	“
77	103	14	44	44	16	40	42	1	55	58	“
78	102	14	52	6	16	39	25	1	47	19	“
79	101	14	59	7	16	37	51	1	38	44	“
80	100	15	5	47	16	35	59	1	30	12	“
81	99	15	12	25	16	33	50	1	21	25	“
82	98	15	18	50	16	31	26	1	12	36	“
83	97	15	25	23	16	28	44	1	3	21	“
84	96	15	31	6	16	25	47	“	54	41	“
85	95	15	36	44	16	22	34	“	45	50	“
86	94	15	42	31	16	19	6	“	36	35	“
87	93	15	48	4	16	15	20	“	27	16	“
88	92	15	53	3	16	11	23	“	18	20	“
89	91	15	57	59	16	7	9	“	9	10	“
90	90	16	2	50	16	2	50	“	“	“	“

VARIATION ON THE EQUATOR.

Angle <i>b a d</i> Fig. 1.	Angle <i>d a c</i> Fig. 1.	Variation East.			Variation West.			Difference.			Sign.
°	°	°	′	″	°	′	″	°	′	″	
91	89	16	7	9	15	57	59		9	10	east
92	88	16	11	23	15	53	3		18	20	"
93	87	16	15	20	15	48	4		27	16	"
94	86	16	19	6	15	42	31		36	35	"
95	85	16	22	34	15	36	44		45	50	"
96	84	16	25	47	15	31	6		54	41	"
97	83	16	28	44	15	25	23	1	3	21	"
98	82	16	31	26	15	18	50	1	12	36	"
99	81	16	33	50	15	12	25	1	21	25	"
100	80	16	35	59	15	5	47	1	30	12	"
101	79	16	37	51	14	59	7	1	38	44	"
102	78	16	39	25	14	52	6	1	47	19	"
103	77	16	40	42	14	44	44	1	55	58	"
104	76	16	41	40	14	37	20	2	4	20	"
105	75	16	42	22	14	29	46	2	12	36	"
106	74	16	42	45	14	22	3	2	20	42	"
107	73	16	42	49	14	14	7	2	28	42	"
108	72	16	42	34	14	6	2	2	36	32	"
109	71	16	42	"	13	57	47	2	44	13	"
110	70	16	41	6	13	49	22	2	51	44	"
111	69	16	39	52	13	40	48	2	59	4	"
112	68	16	38	19	13	32	4	3	6	15	"
113	67	16	36	25	13	23	12	3	13	13	"
114	66	16	34	9	13	14	10	3	19	59	"
115	65	16	31	34	13	5	"	3	26	34	"
116	64	16	28	36	12	56	40	3	31	56	"
117	63	16	25	18	12	46	13	3	39	5	"
118	62	16	21	37	12	36	38	3	44	59	"
119	61	16	17	34	12	27	5	3	50	29	"
120	60	16	13	9	12	17	4	3	56	5	"
121	59	16	8	21	12	7	5	4	1	16	"
122	58	16	3	10	11	57	8	4	6	2	"
123	57	15	57	36	11	46	45	4	10	51	"
124	56	15	51	38	11	36	24	4	15	14	"
125	55	15	45	17	11	26	7	4	19	10	"
126	54	15	38	31	11	16	20	4	32	11	"
127	53	15	31	22	11	4	40	4	26	42	"
128	52	15	23	48	10	54	3	4	29	45	"
129	51	15	16	"	10	43	9	4	32	51	"
130	50	15	7	26	10	32	9	4	35	17	"
131	49	14	58	38	10	21	3	4	37	35	"
132	48	14	49	24	10	9	41	4	39	43	"
133	47	14	39	45	9	58	53	4	40	52	"
134	46	14	29	42	9	47	"	4	43	42	"
135	45	14	19	12	9	35	31	4	43	41	"

VARIATION ON THE EQUATOR.

Angle b a d Fig. 1.	Angle d a c Fig. 1.	Variation East.			Variation West.			Difference.		Sig.
°	°	°	'	"	°	'	"	°	'	
136	44	14	8	16	9	24	7	4	44	9 east
137	43	13	56	55	9	12	18	4	44	37 "
131	42	13	45	8	9	"	34	4	44	34 "
138	41	13	32	56	8	48	45	4	44	11 "
140	40	13	20	17	8	37	1	4	43	16 "
141	39	13	7	13	8	25	2	4	42	11 "
142	38	12	53	43	8	12	49	4	40	54 "
143	37	12	39	47	8	"	42	4	39	5 "
144	36	12	25	25	7	48	31	4	36	54 "
145	35	12	10	38	7	36	16	4	34	22 "
146	34	11	55	25	7	23	48	4	31	37 "
147	33	11	39	48	7	11	33	4	28	15 "
148	32	11	23	45	6	59	6	4	24	39 "
149	31	11	8	14	6	46	36	4	21	38 "
150	30	10	50	25	6	34	2	4	16	23 "
151	29	10	33	8	6	21	25	4	11	43 "
152	28	10	15	28	6	8	44	4	6	44 "
153	27	9	57	24	5	56	"	4	1	24 "
154	26	9	38	57	5	43	14	3	55	43 "
155	25	9	20	7	5	30	25	3	49	42 "
156	24	9	"	55	5	17	33	3	43	22 "
157	23	8	41	20	5	4	19	3	37	1 "
158	22	8	21	26	4	51	41	3	29	45 "
159	21	8	1	12	4	38	44	3	22	28 "
160	20	7	40	36	4	25	40	3	14	56 "
161	19	7	19	41	4	12	36	3	7	5 "
162	18	6	58	27	3	59	31	2	58	56 "
163	17	6	36	57	3	46	23	2	50	34 "
164	16	6	15	8	3	33	13	2	41	55 "
165	15	5	53	4	3	20	2	2	33	2 "
166	14	5	30	44	3	6	49	2	23	55 "
167	13	5	8	10	2	53	35	2	14	35 "
168	12	4	54	22	2	40	19	2	5	3 "
169	11	4	22	20	2	27	42	1	55	18 "
170	10	3	59	8	2	13	3	1	45	25 "
171	9	3	35	54	2	"	24	1	35	21 "
172	8	3	12	11	1	47	4	1	25	7 "
173	7	2	48	29	1	33	43	1	14	46 "
174	6	2	24	40	1	20	20	1	4	20 "
175	5	2	"	43	1	6	58	"	53	45 "
176	4	1	36	42	"	53	36	"	43	6 "
177	3	1	12	35	"	40	12	"	32	23 "
178	2	"	48	25	"	26	48	"	21	37 "
179	1	"	24	14	"	13	25	"	10	49 "
180	"	"	"	"	"	"	"	"	"	"

With the angles in the two first columns of the preceding tables, used respectively according to the formula before given, the quantities of variation noted in the table were obtained. Whence we perceive two different series of variation would, according to the reasoning adduced, present themselves on the equator; one deriving an existence in reference to the north magnetic pole, and the other from the south magnetic pole. That these quantities must influence each other, has been before said, and the simplest and most direct influence is derived from supposing them to destroy each other to an extent commensurate with their respective magnitudes; and hence that the difference between these quantities, with the sign of the largest quantity, would be the actual variation found by observation on the equator. Admitting this to be the case, we have noted down this difference with the proper sign it would receive under such a supposition. And it would appear that in departures from the northern magnetic meridian *a b*, fig. 1, a small quantity of westerly variation would appear, which would increase until it attained its maximum, when it would decrease and become nothing on a meridian 90° east of the northern magnetic meridian. Continuing to proceed east from that meridian, small quantities of easterly variation would appear, which would increase, attain their maximum, and become nothing on a meridian 180° east of the northern magnetic meridian *a c g*; on which meridian the southern magnetic pole *o* is situated; from which it would appear there are four places on the equator, situated at 90° distant from each other, where there would not be any variation.

We have before observed that opposite hemispheres possess opposite characters in variation, and that it is probable they influence each other in regions beyond the equator. It is not impossible that they occasion those curves under which variation appears to arrange itself in either hemisphere, and without doubt cause local attraction in shipping to be a variable quantity. Hence the advantage which would result, in observations such as we have suggested, on local attraction in shipping.

On shore some knowledge respecting the influence of opposite characters in variation in opposite hemispheres might be obtained in a course of observations on the variation upon the equator. A favourable place for these observations presents itself on the South American continent between the Villa Nova at the entrance of the Marañon and Quito. On a line between these places observations on the variation might be made directly on the equator, and in parallels three or four degrees to the north and south of the equator, from which observations it is reasonable to hope some view might be ob-

tained respecting the influence of variation with opposite characters in opposite hemispheres. The space between Villa Nova and Quito, on the South American continent being between 50° and 80° west longitude, would be in the vicinity of the northern magnetic meridian, which we consider in the year 1836 will be situated in $102^{\circ} 38' 21''$ west longitude. Here if the influence of the southern pole be such as we have supposed, small quantities of westerly variation would be perceived. On the other hand, on the African continent, observations might be made, though with some difficulty perhaps from the natives; however, if they could be made, and our views respecting the influence of the southern pole be correct, easterly variation would be discovered in as much as the southern magnetic meridian acg in 1836 will be situated in $77^{\circ} 21' 39''$ east longitude; for $102^{\circ} 38' 21'' + 77^{\circ} 21' 39'' = 180^{\circ}$ or the distance between the meridians abg and acg ; consequently that portion of Africa which lies on the equator, being between 10° and 40° east longitude, is in the neighbourhood of the southern magnetic meridian or acg .

Observations made, then, upon the equator, on these two continents would be calculated to develop the influence which opposite characters in variation in opposite hemispheres may maintain, and ultimately give rise to that correction, which observations made on shore appear to require. At sea it is probable this influence (whatever may be its ratio of action) passes into that which occasions local attraction to be a variable quantity, and it may be hoped that its nature would be discerned in a series of experiments on local attraction such as we have suggested.

No remark need be offered on the beneficial results which might be hoped for in developing the laws by which the deflections of the needle are regulated. The attention which has been so long bestowed by the public at large, is at once an answer to any enquiry that could be made on that subject. On the other hand much might be urged as to why, after so much attention has been paid to it, the enquiry should not be prosecuted. The discoveries as to the identity between electricity and magnetism present a strong inducement as that identity connects the subject with other branches of science. But all this may be admitted, and the interest taken by the world allowed, and it may yet be contended, that the views set forth in the preceding pages will not aid the enquiry. It may be said that no physical reason can be given why the needle should be attracted to any point on the arctic circle; and until one can be given, consequences derived from its assumption deserve no consideration.

To this it may be modestly replied, if at present it be impossible to assign any reason why the needle should be attracted to a point in the arctic circle, derived from the pole of the ecliptic, the assumption of such a fact has cleared the subject of variation of much of its former obscurity; and therefore ought not to be despised on the grounds of its being an assumption. Scientific enquiry does not disdain even false assumption; some of the most elegant and conclusive demonstrations in mathematics owe their existence to it. Logic gratefully acknowledges its use; and human reasoning on all the affairs of life, without it, would be less acute; but in scientific research utterly useless. That the needle is attracted to a point on the arctic circle, has nothing in it whereby it should be condemned: it may be despised if it be an inadequate assumption, or one if properly examined, which proves the contrary of that which it is brought forward to prove; but this cannot be urged against it. On the contrary, as before said, it clears the subject of variation of much of its obscurity, and causes the deflections of the needle to appear with that order and regularity which all the explored laws of nature appear to follow. On the other hand, the assumption may be condemned, if it can be proved to have been taken up and made to agree with discoveries otherwise derived; but this cannot be said. The assumption was made and communicated to the Right Hon. the Lords Commissioners of the Admiralty, in 1815. It has not been changed throughout the whole period of the polar voyages, was confirmed in the first of those voyages, Capt. Ross's first voyage; and the magnetic meridian shown to be in motion, and this with so much apparent accuracy, that a mean annual motion of that meridian, derived from the observations of that voyage, has since only been used, in all cases, where the variation has been computed, according to the principles of the assumption as laid down in the preceding pages. It may also be stated that in Capt. Parry's first voyage the assumption received a further confirmation as may be seen in that which is herewith submitted; and moreover it has enabled the author to do what no other person has ever attempted, namely, submit all the observed quantities of Capt. Parry's first voyage to a comparison with one particular observation made in England at the earliest period that variation was known; and thereby deduce more regularity in the deflections of the needle than has ever been proved to exist under any other test of examination. It may also be added that the assumption is not at variance, or incompatible, with any of the known phenomena of the earth's motion. And although it may at present be impossible to give a physical

reason why the needle should be attracted to any point on the arctic circle, derivable from the pole of the ecliptic, yet that recent discoveries, as to the identity between electricity and magnetism, together with what is known on the polarization of light, would render it probable that the time may not be distant when it shall be possible for science to assign a physical reason why the needle should be so attracted.

With a view of glancing at this, and not with the intention of making any assertion on the subject, I may, in conclusion, be permitted to remark, that, whatever be the properties of light, it either emanates from the sun or tends to the sun as its centre; and that, whatever may be its component parts, it is a fluid. Also, that the earth in its motion round the sun, passes through this fluid with a double motion, one rotatory on its axis, and the other, that by which the earth advances in its orbit. Now, if any change be effected in light, by the passage of the earth through it, and the earth can be supposed to be a recipient of any effect, under those changes, it would probably be discernable, under peculiar circumstances upon the arctic and antarctic circles. This remark has been offered only with a view of showing that there does not appear any one single circumstance in the phenomena of the earth's motion which can be imagined as a reason against the possibility of the needle being attracted to a point on the arctic circle; and therefore it is hoped their Lordships will be pleased to give the subject that consideration which its importance appears to require.

EDWARD NAYLER.

Circus, Southsea, Feb., 19, 1839.

XIII. *Investigations in Magnetism and Electro-magnetism.*

By JAMES P. JOULE, ESQ. In two letters to the Editor.

Broom Hill, near Manchester, May 28, 1839.

Dear Sir,

I am now able to send you an account of my further investigations on electro-magnetic attraction. It was judged to be a matter of the first importance, in a research like the following, to have a galvanometer, the indications of which might be depended upon.

Fig. 11, Plate III, represents the plan of my galvanometer; n is the needle, 2 inches long. The wire is 10 feet long, and 1-16th of an inch in diameter; it is disposed in four coils, and cups are placed at the parts of the wire marked a, b, c, d, e . The wire crosses at x, x , in every other part it is in the

same plane. It will be seen that by this contrivance the forces of the several coils are made equal, or as it seems not perceptibly different.

The process of graduation was conducted in the following manner. The electricity of a constant battery was first passed through each of the coils, and the deflection of the needle was observed to be equal for each. A current of a certain intensity was then passed from *a* to *b*, *a* to *c*, *a* to *d*, and *a* to *e*,* and I marked the several deviations of the needle on the card of the instrument 1, 2, 3, 4. I then increased the power of the battery until the needle stood at 2, when the current passed from *a* to *b*; the former process was then repeated, and I marked on the card 2, 4, 6, 8; and going on in this manner I obtained the numbers 1, 2, 3, 4, 6, 8, 9, 12, 16, &c. When the galvanometer is used the current of electricity is passed from *a* to *b*, and the above numbers represent absolute quantities of electricity.

In order to give you a definite idea of the quantities of electricity indicated by my galvanometer, I took a diluted acid, consisting of 10 parts of water, and 1 part of sulphuric acid sp. gr. 1.8, and passed through it a current = 1. In seven minutes, 62 cubic inches of the mixed gases were produced. The electrodes were pointed platina wires 1.1 inch asunder.

The magnets used below were those described in my last.† They are straight and square, 7 inches long, and wound with 22 feet of copper wire 1-16th of an inch in diameter. Five of them are of solid iron, and five corresponding ones of square iron wire. The sides of the square sections of Nos. I, are 3-11th of an inch, and this dimension is successively increased by 1-11th until Nos. V, which are 7-11ths of an inch.

The iron magnets were successively suspended to the beam of a balance, and the corresponding wire magnets were brought underneath, so that 1-8th of an inch intervened between the poles of the two magnets. The electricity of forces exhibited in the table was passed in one current, through the wires of the magnets, and the coil of the galvanometer. The attraction is measured in grains.

* I took care to decrease the resistance of the battery wires in proportion as the length of that part of the galvanometer wire through which the current passed was increased.

† See page 58. of the present volume. Edit.

TABLE I.

Quantities of Electricity.	<i>Magnets.</i>				
	I	II	III	IV	V
6	76	65	88	62	42
8	133	100	180	103	98
12	258	296	300	286	206
16	500	548	530	550	410
24	1080	1280	1190	1210	1050

In order to vary these experiments, and with a view of ascertaining what effect an increase of length would produce, I constructed ten more electro-magnets of the same sectional areas, but 14 inches long, or double the former length, and wrapped with 22 yards, or three times the length, of similar wire. I and II were made of square iron wire, the rest of solid iron.

TABLE 2.

Quantities of Electricity.	<i>Magnets.</i>				
	I	II	III	IV	V
8	410	667	1150	1205	1175
corrected	675	990			
12	690	1170	2150	3025	2625
corrected	1080	1740			
16	1000	1920	4575	5687	4675
corrected	1460	2710			
24	1460	3500	9625	11812	10500
corrected	2080	4750			

Every one of the magnets used above (except Nos. I and II in the second table) was wrapped to two thicknesses by the wire, and in the large ones the iron was left uncovered at equal intervals. I must mention, however, that Nos. I and II in the last table were wrapped to three thicknesses in some parts on account of their small size; and on calculation I find that on this account the attractions under those numbers should be corrected as in the table.

It does not appear from the results of these experiments that there is any great detriment in the increase of the length

of the iron. It is plain that, as the magnets in table 2 are wrapped with three times the length of wire, 24 of electricity in the first table should have the same effect as 8 in the second table. The difference is due to the increased length of iron. I do not think myself justified in assigning any amount to this difference, which, however, seems to increase in value as the section of the magnets decreases. In order to determine this and many other circumstances of great interest, it would be necessary to conduct these experiments in a much more comprehensive manner than I have done; and particularly to examine minutely into the various powers of hard and soft iron and wire.

I think, however, that I have by these experiments discovered a most important law, namely: *The attractive force of the electro-magnet is directly as the square of the electric force to which its iron is exposed: or if E denote the quantity of electricity, M, the magnetic attraction, and W the length of wire, $M = E^2 W$.*

It must be confessed that there are many instances in the above tables which seem to form exceptions to this law. I consider, however, that the effects of magnetic inertia, and the sources of error which I found it impossible to avoid, are sufficient to account for these; perhaps the fairest way of comparing the theory with experiment, is to take the means of all the numbers in the first table; and the means of III, IV, V, in the second; omitting I and II, because it is clear that they are at last becoming saturated with magnetism. Here are the means of the numbers obtained by experiment, and against them are the calculated results.

From 1st Table.

From 2d. Table.

Electricity.	Experiment.	Calculation.	Experiment.	Calculation.
6	66.4	66.4		
8	123	118	1177	1177
12	269	265	2600	2648
16	508	472	4979	4708
24	1163	1063	10646	10593

Anxious to know whether this law obtained in lifting, as well as in distant attraction, I made the following rough experiment. I used a horse-shoe electro-magnet made of a cylinder of iron, 7 inches long, 5-8ths of an inch in diameter, and wound with 5 yards of thick copper wire. The law seems in this case to fail principally because the iron is sooner

saturated with magnetism; hence the propriety of making electro-magnets for lifting of considerable bulk.

Electricity.	Lifting power in lbs.	Calculation.
4	3.5	3.5
6	6.5	8.0
8	11.5	14.0
12	21	31.5

It is surprising to see how alike the attractions of the different electro-magnets are when exposed to the same electric force. From this I infer that little difference of effect would be produced, of whatever shape or size the sectional area of the magnets might be, provided that their weight and length remained the same.

I can scarcely doubt that electro-magnetism will eventually be substituted for steam in propelling machinery. If the power of the engine is in proportion to the attractive force of its magnets, and if the attractive force is as the squares of the electric force, the economic effect will be in the direct ratio of the quantity of electricity, and the cost of working the engine may be reduced ad infinitum. It is, however, to be determined how far the effects of magnetic electricity may disappoint these expectations.

I find that the plan which I had proposed to myself for a new engine must yield to the views elicited by these experiments. As far as I can see at present I think it will be best to use only two very large electro-magnets, and to expose those two to all the electric power I can command.

I remain, dear Sir,

Yours truly,

JAMES P. JOULE.

Broom Hill, near Manchester, July 10, 1839.

Dear Sir,

The following experiments were designed for the "experimentum crucis" of the theory given in my last letter. I have here measured the magnetic attractions at various distances, and flatter myself that the results are not unworthy of your notice.

Two pairs of electro-magnets were constructed. Each of the first pair was made of iron, 30 inches long and 1 inch square; each of the second pair 30 inches long, 2 inches broad, and 1 inch deep. The sharp edges were ground down to prevent inconvenience in the winding of the wire. Each mag-

net was properly insulated and wound with 88 yards of covered copper wire 1-16th of an inch in diameter.

The attractions were measured in precisely the same manner as before, saving the substitution of copper for wood to keep the magnets at the proper distance apart. The attraction of the suspended magnet for the fixed one is measured in ounces, avoirdupois.

		Electricity	6	8	12	16	24	32
First Pair.	at $\frac{1}{8}$ inch	Experiment	18	33	72	124	260	
		Theory	18	32	72	128	288	
	at $\frac{1}{4}$ inch	Experiment	7	13	28	47	96	
		Theory	7	12.44	28	49.7	112	
	at $\frac{1}{2}$ inch	Experiment	3	5.25	12	18	38	62
		Theory	3	5.33	12	21.3	48	85.3
Second Pair.	at $\frac{1}{8}$ inch	Experiment	14	27	60	100	240	
		Theory	14	25	56	100	224	
	at $\frac{1}{4}$ inch	Experiment	6.25	12	25	40	96	
		Theory	6.25	11.1	25	44.4	100	
	at $\frac{1}{2}$ inch	Experiment	2.5	5	9.5	17.5	36	
		Theory	2.5	4.44	10	17.7	40	

The experimental results are quite as near to the theoretical as I can expect,* considering the several sources of error which are liable to interfere. Those belonging to the first pair are particularly satisfactory, especially if, with regard to the numbers under 16, 24, and 32, we make an allowance for the saturation of their iron.

I inferred from the experiments detailed in my last letter, that little difference of power would result from the mere alteration of the shape of the sectional area of the iron of the electro-magnet; that view is confirmed by the experiments just related, in which it will be seen that little difference exists between the magnetic powers of the first and second pairs, and even that difference may be partly accounted for, when we take into account the difficulty of winding the covered wire closely to the sides of broad rectangular iron.

These magnets were wound to two thicknesses by the covered wire, and in other respects were similar to those I have before used: we can therefore estimate the effect arising from increase of length. These magnets, 30 inches long, with 88 yards of wire, and (6) electricity, sustained at 1-8th of an

* I am sorry that circumstances prevented me from repeating the experiments once or twice. If I had done so, and taken the means, I should certainly have come much nearer.

inch distance the mean weight 7000 grs.; whilst the mean sustaining power of numbers 3, 4, 5, in my last, was with the same electric force, or 22 yards of wire, and (24) electricity, 10646 grs.

I have made some experiments which show that the law of the attraction of straight steel magnets for each other, is in the inverse ratio of the distances whenever the length of the magnets, compared with their distances asunder, was so great as to render insensible the action of the distant poles. On examining the above table the same ratio will be found to hold good with regard to the electro-magnets, with a modification which is probably entirely owing to the increased development of magnetism in consequence of the approach of the electro-magnets towards one another.

I remain, Dear Sir,

Yours most respectfully,

J. P. JOULE.

XIV. *Experimental and Theoretical Researches in Electricity, Magnetism, &c. Third Memoir.* By WILLIAM STURGEON, Lecturer on Experimental Philosophy at the Honourable East India Company's Military Seminary, Addiscombe, &c.

On the direct action which caloric exercises on magnetic poles.

138. The cause of the phenomena of terrestrial magnetism, notwithstanding the close application of much philosophical talent to the subject, still remains concealed within a dense cloud of mystery, penetrable, perhaps, only by progressive steps of development, between which long periods of time may possibly intervene. The discovery of electro-magnetism, however, has done much to tranquillize the minds of philosophers to one general opinion respecting the grand source of terrestrial magnetic action; and the appearance of thermo-electricity has given a sanction to the hypothesis which, independently, might never have been conferred on it. But whether the *main* phenomena of terrestrial magnetism be due to circumflowing electric floods or to some other grand physical cause, there are certainly some singular *minor* phenomena which may probably be traced to the influence of other natural agencies; and, independently of any electric currents which it may be supposed to excite, solar heat alone, appears to me to play a very active part, and especially in the diurnal variations. I am well aware that this opinion is far from being a

novel one; it is indeed the opinion of many philosophers of the present day, and although not much supported either experimentally or otherwise, I believe it to be generally acknowledged by all those who have paid a due attention to the phenomena. With the exception of the interesting experiments of M.M. Coulomb, Barlow, and Christie,* I know of none that have been devoted directly to the elucidation of magnetic phenomena under various temperatures; and although the experiments I am about to describe are of a very different description to any recorded by those philosophers, and have afforded perfectly novel results, and developed unsuspected relations of heat to magnetism, which to me have appeared to bear directly on the subject, I cannot pretend to say what may be their aspect to others, nor how far philosophers may be disposed to apply them to the solution of the curious and hitherto intricate problem which the needle's diurnal variations have so long presented to their notice.

139. The experiments, as has already been stated, (114 note)† originated in a search for thermo-electric currents in a permanent steel magnet, and they were carried on in the following manner. A magnetic needle, four inches long and furnished with an agate cap supported on a fine steel point in the centre of a graduated circle, was placed on a firm table, and the meridian line of the card properly adjusted to the vertical plane passing through the poles of the needle. In a horizontal plane, four inches higher than that in which the needle was situated, and 12 inches eastward of the needle's pivot, was placed a flat bar magnet of steel. The dimensions of the bar are eight inches long by one inch broad, and about a quarter of an inch thick. It was well hardened and magnetized, and when placed nearly parallel to the magnetic meridian, having its centre opposite to the pivot of the needle, and its marked pole southward, the south end of the needle was drawn towards the magnet $2\frac{1}{2}^{\circ}$; a position selected for it to stand in

* Subsequently to the reading of this memoir I observed in Dr. Brewster's *Treatise on Magnetism* that Prof. Kupffer had also made some experiments of this kind, and had ascertained some important facts, similar to a few of those which I met with in this investigation. But as the mode by which I proceeded is very different to that pursued by Prof. Kupffer, and as several of the facts which have been developed by my experiments have not been met with by that philosopher, I have not deemed it necessary to alter the original form of this memoir.

† See page 46 of this volume.

until the heat of a spirit lamp should be applied to the magnet.*

140. The needle being thus adjusted and perfectly at rest, the flame of a spirit lamp was placed directly under the marked end of the magnet, and in such a position as it might heat the polar point, which is always situated at some distance (half an inch or more in some magnets) from the extremity of the bar, to the greatest extent. In a very short time the needle was observed to move in consequence of the heat varying the magnetic action of the bar; but instead of its south end, which was nearest to the heated pole of the magnet, receding to the meridian, as I had been led to expect, it soon showed its tendency to move the other way, and to increase the angle of deflection. This novel result led me to continue the heat for some considerable time, and until the heated end of the magnet had passed through all the shades of colour from that of pale straw to pale blue, and eventually the needle marked an angle of 6.5° , having gained 4° of deflection by the heating process. The lamp was now removed from the magnet, and in one minute afterwards the needle was observed to recede again, and the angle progressively diminished as the bar became of a more equable temperature throughout; and when the general temperature had again subsided to the original standard the needle rested at a deflection of 4° , being 1.5° more than the original angle prior to the application of the lamp. This experiment was repeated by again placing the lamp under the marked end of the magnet until the needle again marked an angle of 6.5° . When the magnet had cooled to its first temperature the angle of deflection was again 4° . Hence the results in both experiments were precisely the same; at least the *extreme* and *terminal* angles of deflection were respectively the same as the extreme and terminal angles in the former experiment.

141. The lamp was now placed under the *unmarked* pole of the magnet, and shortly afterwards the needle was observed to move towards the meridian line of the card. In about ten minutes the deflection was 0° , but the needle could not be made to pass that point, although the lamp was kept under the magnetic pole for fifteen minutes, at the expiration of which time it was removed. The needle shortly after the removal of the lamp began to return to its previous deflection,

* This position of the needle was afterwards found to be of no particular use, and after a few preliminary experiments the *magnetic meridian* was chosen for the first position of the needle in each experiment of this kind.

and when the magnet had been restored to its original temperature the needle came to rest at 2.5° , or precisely in its original position prior to the application of heat in the first experiment.

142. The results of the above experiments will be more conspicuously exhibited in the following tabulated form. The deflections are given for the north end of the needle.

First Experiment.

The magnet 4 inches above the level of the needle parallel to it, and its centre 12 inches to the eastward of the needle's pivot. See the whole arrangement in fig. 1, Pl. III. The dotted needle is the new position taken by the compass needle when the lamp is applied to the marked pole of the magnet.

Deflections.			
Before the magnet was heated.	2.5° W	} Transient	} = 4.0°
Marked pole heated	6.5 W		
When the magnet had returned to its original equable temperature	4.0° W		
Original deflection	2.5 W	} Permanent	} = 1.5°

Second Experiment. (Fig. 2.)

The situation of the magnet as in the first experiment.

Deflections.			
Before the magnet was heated .	4.0° W	} Transient	} = 4.0°
Unmarked pole heated. . . .	0.0 W		
When the magnet had returned to its original equable temperature	2.5° W	} Permanent	
Original deflection. . . .	4.0 W		} difference

143. There is something singularly uniform in the results of these two experiments, from which it would appear that, when the original magnetic force is disturbed by an augmentation of temperature at one extremity of the bar much above that of the other by any quantity F , the resulting *permanent* force f , will always be in a certain ratio with it. In both the above experiments, if we assume the deflections of the needle to be due to the introduction of a new force into the magnet, the *transient* new force F is to the resulting *permanent* new force f as $4 : 1.5$, or at least as nearly so as those arcs are to their sines, which in this case would be a tolerable approximation. Now, from whatever cause the variation in the magnetic intensity may have immediately proceeded, they obviously originated from the agency of caloric as a disturbing force, whose action on the magnet, even by a diversity of effects,

might, directly or indirectly, be productive of the phenomena exhibited by the needle. For instance, the phenomena might obviously be explained by admitting any of the following effects being produced in the magnet by the action of caloric. By an augmentation of power in the heated pole; by a diminution of power in the cooler pole; by an augmentation of power in both poles, but to the greatest extent in the heated one; by a diminution of power in both poles, but greatest in the cooler one; by a change of position, in the bar, of either north or south pole, or of both of them. Under any of these circumstances the deflections of the needle might obviously have occurred, and perhaps there are some others which the heat might have occasioned in the magnet that would have been productive of similar deflections. I thought also of the effects which thermo-electric currents might produce on the needle provided their existence could be admitted; and although the permanent effects which were left on the magnet could not be easily reconciled to their action, the *transient* deflections having an obvious dependence on the point of heat required further experimental investigation to ascertain whether the presence of thermo-electric currents were, or were not, the cause. For this purpose three other stations were selected for the magnet, and experiments, similar to those already described, were made at each station. The magnet was brought closer to the needle in expectation of the effects being increased. The results are arranged in the following tables.

Third Experiment. (See Fig. 1.)

The magnet re-magnetized 4 inches below the level of the needle and 6 inches to the east of it. Marked pole southward.

		Deflections.	
Before the magnet was heated .	2.0 W	} Transient	} = 8.5.
Marked pole heated	10.5 W		
When the magnet had returned to its original equable temperature	6.0 W	} Permanent	} = 4.
Original deflection . . .	2.0 W		

Fourth Experiment. (See Fig. 2.)

The situation of the magnet as in the third experiment.

Deflections.			
Before the magnet was heated .	6.0° W	} Transient	} = 8.5.
Unmarked pole heated . . .	2.5 E		
		} difference	
When the magnet had returned to its original equable temperature	2.0° W	} Permanent	} = 4.
Original deflection . . .	6.0 W		
		} difference	

144. *Fifth Experiment. (Fig. 3.)*

Magnet nearly parallel to the magnetic meridian ; 4 inches below the level of the needle, and 6 inches westward of it. Marked pole of the magnet southward. Its force was that left by the last experiment.

Deflections.			
Before the magnet was heated .	2.0° W	} Transient } difference	} = 4.5°
Marked pole heated	2.5 E		
When the magnet had returned to its original equable temperature	1.75 W	} Permanent } difference	} = 0.25°
Original deflection . . .	2.0 W		

145. *Sixth Experiment. (Fig. 4.)*

The situation of the magnet the same as in the fifth experiment. Not re-magnetized.

Deflections.			
Before the magnet was heated .	1.75° W	} Transient } difference	} = 3.25°
Unmarked pole heated	5.5° W		
When the magnet had returned to its original equable temperature	1.75° W	} Permanent } difference	} = 0°
Original deflection . . .	1.75 W		

145. The results of the fifth and sixth experiments being so very different to any of the preceding ones, it occurred to me that this difference must either arise from the new position of the magnet (west instead of east of the compass needle) or from a want of re-magnetizing. Accordingly these experiments were repeated under precisely the same circumstances as before, with the exception of the magnet being re-magnetized by the application of a horse-shoe magnet. The following are the results of a repetition of the fifth and sixth experiments.

Fifth Experiment repeated. (Fig. 3.)

Magnet re-magnetized.

Deflections.			
Before the magnet was heated .	2° W	} Transient } difference	} = 8.75°
Marked pole heated	6.75 E		
When the magnet had returned to its original equable temperature	2.25° E	} Permanent } difference	} = 4.25°
Original deflection . . .	2° W		

Sixth Experiment repeated. (Fig. 4.)

Magnet not altered from last experiment.

Deflections.			
Before the magnet was heated	2·25° E	Transient	} = 8·25°
Unmarked pole heated	6° W	difference	
When the magnet had returned to its original equable temperature	1·75° E	Permanent	} = 4°
Original deflection	2·25° E	difference	

Seventh Experiment. (Fig. 3.)

Magnet retouched. Situated 4 inches above the needle and 6 inches westward of it. Marked pole southward.

Deflections.			
Before the magnet was heated	2° W	Transient	} = 7·5°
Market end heated	5·5° E	difference	
When the magnet had returned to its original equable temperature	1·75° E	Permanent	} = 3·75°
Original deflection	2° E	difference	

Eighth Experiment. (Fig. 4.)

Magnet not molested since seventh experiment.

Deflections.			
Before the magnet was heated	1·75° E	Transient	} = 7·25°
Unmarked pole heated	5·5° W	difference	
When the magnet had returned to its original equable temperature	1·75° W	Permanent	} = 3·5°
Original deflection	1·75° E	difference	

146. The uniformity which the results of these experiments have manifested is so remarkably exact as almost to surpass belief; and I have myself really been astonished at the rigid accuracy in the corresponding phenomena, as they were successively developed, and over the display of which, I was soon led to discover, I had no possible control, when the circumstances of the experiments were strictly the same. The transient deflections, which appear in the tables, were the greatest I could obtain by the means employed. They invariably increased most rapidly within the first seven minutes after the application of the spirit lamp; and generally attained a *maximum* before the expiration of twelve minutes. A longer continuation of heat would sometimes cause an increase of a quarter of a degree, but hardly ever more, and in no instance could I get an increase of deflection after the ex-

piration of fifteen minutes, although, on some occasions, the lamp was kept burning under the pole of the magnet for more than half an hour.

147. The maximum and minimum deflections due to heat in the last six experiments were so obviously near to the ratio of 2 : 1, throughout the whole series that one would be led to imagine that such is the absolute ratio of the maximum and minimum disturbing forces, or of the changes which took place in the magnet by the action of caloric. Hence, by again representing those forces by F and f respectively, we have

$$F : f :: 2 : 1 \text{ or } F = 2f.$$

which is the remarkably simple law developed by these experiments.

148. It is true that the first results of the fifth and sixth experiments are not conformable to that law ; but it must be remembered that they were not made under the same circumstances as the rest. The magnet had not been retouched since the fourth experiment, and, consequently, was still under the effects, whatever they were, of the process of the two preceding experiments : and it cannot but be interesting to observe the remarkable correspondence of the results of these experiments when repeated, with the magnet retouched, with those of all the rest.

149. The first results of the fifth and sixth experiments, though not in accordance with the same law which is so obviously attached to the others, are not without a due share of interest. They show that there may be a possibility of completely extinguishing these *permanent* effects of heat upon the magnet which are so conspicuously manifested by the other experiments : and they prove to demonstration, that the *transient* effects may be considerably abated by renewed applications of heat to the magnet. But at this stage of the enquiry the phenomena seem to admit of such a diversity of causes for their explication ; and so many thoughts flash across the mind respecting the most eligible mode of pursuit, that it is extremely difficult to choose that experiment which ought to be made first, in preference to any other, for the purpose of arriving the most speedily at conclusive results. The point which appeared most easy to decide was that respecting electric currents which might be supposed to be generated by the action of the applied heat : but if any such currents have existed the phenomena hitherto exhibited by the compass needle have not given any indications of that regularity of their magnetic actions as ought to have been

expected from the known laws of electro-magnetism ; unless, indeed, they have taken very different routes to any previously developed by heat in the simple metals.* For it is obvious that the deflections of the needle were in the *same direction* with respect to the point of heat on the magnet whether the latter was *above* or *below* the level of the needle ; an occurrence not likely to have taken place from the influence of electric currents. Moreover, the *quantity* of action at the distance (6 inches) between the magnet and needle, was much greater than any known thermo-electric current could produce : and the residual *permanent* action could not easily be reconciled to the operation of any such agency. However, to decide this point still more effectually, the magnet was placed on the same horizontal level as the needle and at the same distance (6 inches) from it as before : first on the west and then on the east side of the needle : heat being applied at each station as in the preceding experiments : for it was obvious that if the deflections were due to a *direct* action of caloric on the polar forces of the magnet, independently of an intermediate agency of electric currents, the horizontal action on the needle would be greater than when the magnet was either *above* or *below* that level of the needle. The magnet was retouched and placed parallel to the needle. The following are the results.

Ninth Experiment. (Fig. 3.)

Magnet retouched, and placed 6 inches westward from, and parallel to, the needle—Marked pole southward.

	Deflections.	
Before the magnet was heated .	0°	} Transient } = 11.5°
Marked pole heated	11.5 E	
		} difference }
When the magnet had returned to its original equable temperature }	6.5° E	} Permanent } = 6.5
Original deflection	0	
		} difference }

Tenth Experiment. (Fig. 4.)

Magnet not molested since the last experiment.

	Deflections.	
Before the magnet was heated .	6.5° E	} Transient } = 12°
Unmarked pole heated	5.5 W	
		} difference }
When the magnet had returned to its original equable temperature }	1.5° E	} Permanent } = 5°
Original deflection	6.5 E	
		} difference }

* See my paper on the Thermo-electricity of simple bodies, Phil. Mag. and Annals of Philosophy, Vol. X. p. 1.

Eleventh Experiment. (Fig. 1.)

The magnet retouched, and placed in the same horizontal plane as the needle, parallel to it and 6 inches eastward. Marked pole southward.

	Deflections.	
Before the magnet was heated .	0°	} Transient } = 12.75° } difference }
Marked pole heated	12.75 W	
When the magnet had returned to its original equable temperature	6.5 W	} Permanent } = 6.5° } difference }
Original deflection	0	

Twelfth Experiment. (Fig. 2.)

Magnet not molested since the eleventh experiment.

	Deflections.	
Before the magnet was heated .	6.5° W	} Transient } = 12.5° } difference }
Unmarked pole heated	6 E	
When the magnet had returned to its original equable temperature	0°	} Permanent } = 6.5° } difference }
Original deflection	6.5 W	

150. The results of the four preceding experiments so effectually remove all suspicion of the agency of electric currents being concerned in the production of the phenomena that it would be totally useless to pursue that part of the enquiry any further. The whole series of phenomena obviously depend upon some *direct* action of caloric on the original magnetic forces. The ratio of the transient to the permanent effects so beautifully developed in the preceding experiments, is not so precisely observed by those of the last four; but still the anomalies are not so great as to entirely overrule the probability of the general correctness of the law; indeed the approximation to that law is too close to be otherwise considered than as favourable to its generality, as far at least as the magnet employed is concerned; but similar experiments on other magnets of different magnitudes and other kinds of steel, require to be made before any just conclusions can be drawn respecting the generality of this or any other law connected with this branch of physics. Such a series of experiments however requires more time than I can afford to devote to them at present, though I cannot willingly permit so important a part of the subject to pass by without some further knowledge of it, with a view to which I have made a few other experiments, whose results appear very satisfactory, and corroborative of that law which the first series of experiments developed. The bar magnet used in the four following experiments is of cast steel, seventeen inches long, and weighs a

pound and a half avoirdupoise ; consequently three times the magnitude of the former (139). It was placed first westward and then eastward of the needle, six inches distant on each side, and in the same horizontal plane, having its marked end southward in both stations, and two experiments made in each in precisely the same manner as the four preceding ones. The following were the results.

Thirteenth Experiment. (Fig. 3.)

Magnet 17 inches long, weight 24 avoirdupoise ounces, placed parallel to the needle in the same horizontal plane, having its centre 6 inches westward and marked pole southward.

	Deflections.	
Before the magnet was heated .	0°	} Transient } = 14.5° } difference }
Marked pole heated	14.5 E	
When the magnet had returned to its original equable temperature	6.75° E	} Permanent } = 6.75° } difference }
Original deflection	0.0	

Fourteenth Experiment. (Fig. 4.)

Magnet not molested since the thirteenth experiment.

	Deflections.	
Before the magnet was heated .	6.75° E	} Transient } = 17.75° } difference }
Unmarked pole heated	11.0 W	
When the magnet had returned to its original equable temperature	3° W	} Permanent } = 9.75° } difference }
Original deflection	6.75 E	

Fifteenth Experiment. (Fig. 1.)

The magnet was retouched and placed 6 inches eastward of the needle and in the same horizontal plane as in the two preceding experiments.

	Deflections.	
Before the magnet was heated .	0°	} Transient } = 16° } difference }
Marked pole heated	16 W	
When the magnet had returned to its original equable temperature	8.75° W	} Permanent } = 8.75° } difference }
Original deflection	0	

Sixteenth Experiment. (Fig. 2.)

Magnet not molested since the fifteenth experiment.

	Deflections.	
Before the magnet was heated .	8.75° W	} Transient } = 17.75° } difference }
Unmarked pole heated	9.0 E	
When the magnet had returned to its original equable temperature	0.75° E	} Permanent } = 9.5° } difference }
Original deflection	8.75 W	

151. It will be observed, in the last three experiments, that the resulting *permanent* deflection is something more than one half of the *transient* deflection; but that in the preceding one (13th.) it is something less than one half: but in no case does there appear an aberration of more than three quarters of a degree; and as this is sometimes on one side and sometimes on the other, the formula $F = 2f$ appears to be the nearest approximation to the true law that these experiments are susceptible of communicating. If indeed we take the sines of the angles as the representatives of the new deflecting forces, the ratio of the maximum to the minimum would be somewhat different. By taking the means of the sines of the angles of deflection due to the new condition of force with the large magnet the formula would become

$$F = 2f + \frac{F}{31.4} \dots \dots b;$$

and by proceeding in a similar manner with the means of the sines of the angles developed by 9th, 10th, 11th, and 12th experiments, with the smaller magnet, the formula becomes

$$F = 2f + \frac{F}{20.3} \dots \dots c.$$

152. In both these cases the resulting permanent forces are somewhat greater than one half of the maximum transient forces; but as the excess above one half is so very trifling a quantity, especially for the smaller magnet, which amounts to only $\frac{1}{11}$ of the maximum force, it may fairly and conveniently be neglected, being allowable for probable errors of observation; and although the formula *b* exhibits a greater aberration than formula *c* from our first formula *a*, the latter does not appear to be very far from the truth when errors of experiment are allowed for. These formulæ, however, are of no farther use than that of exhibiting a singular coincidence in the various experiments of the new conditions of the deflecting magnetic forces; having no reference whatever to the cause of them.

153. I have made a few experiments with very small magnets whose results have deviated but very little from the general law already pointed out. (147, 151.) I will state the results of two of these experiments which are a fair specimen of all the rest. The magnet was of cast steel, five inches long, and weighed about one ounce and a quarter. It was newly magnetized and placed in the same horizontal plane as the needle, and parallel to it, and four inches westward, with its marked pole southward.

Seventeenth Experiment. (Fig. 3.)

Deflections.			
Before the magnet was heated	. 0°	} Transient } } difference }	= 3.0.
Marked pole heated	. . . 3 E		
When the magnet had returned to its original equable temperature	} 1.5° E	} Permanent } } difference }	= 1.5°
Original deflection	. . 0		

Eighteenth Experiment. (Fig. 4.)

Magnet not molested since last experiment.

Deflections.			
Before the magnet was heated	. 1.5° E	} Transient } } difference }	= 3.5°
Unmarked pole heated	. . . 2 W		
When the magnet had returned to its original equable temperature	} 0.25° W	} Permanent } } difference }	= 1.75.
Original deflection	. . 1.5 E		

154. In the experiments with the small five-inch magnet, the maximum deflection was usually attained in three or four minutes, the needle never advancing after the fourth minute; but on the other hand, it would sometimes return half a degree or more, and as I have never yet seen an instance of the deflection increasing after such retrograde movement, it is a good indication of the angle having attained its maximum. The maximum deflection with the large eighteen-inch magnet, was attained, usually, in twenty-five minutes; forty minutes' duration of heat never increased the deflection above that attained in twenty-five, nor lessened it more than half a degree. With the small five-inch magnet, ten minutes' continuation of the lamp would sometimes reduce the maximum angle 2° or more: but in no instance have I seen the needle return to its first position during the application of heat; but it has sometimes stood, after the magnet had returned to its original equable temperature, at the same angle as the lamp left it. Hence, the results which are so uniformly exhibited by large magnets, are not to be obtained in very small ones, unless the lamp be removed at the precise moment of maximum deflection; under which circumstances, the phenomena appear to be regulated by the same laws in all those hitherto tried; and probably in all other bar magnets of the usual shape, which weigh more than two ounces, and treated in the manner I have described: that is, by placing the lamp under the poles, and allowing the flame to play close to the ends of the bar during the whole time of its application.

155. The laws which I derive from these and other similar experiments,* are simply as follow.

First Law. *The pole of each magnet which receives caloric, acquires an ascendancy of deflecting force to a certain amount, over the other pole; which ascendancy is at a maximum during the supply of caloric.*

Second Law. *When the supply of caloric ceases, the ascendancy of force, acquired by the receiving pole, lessens; and eventually subsides to about one half of the maximum ascendancy, and retains the latter ascendancy until some extrinsic agent again disturbs the polar forces of the magnet.*

156. There seems to be a capriciousness in the phenomena exhibited by a thin strip of steel, such as a lady's busk, not discoverable in thicker pieces. The busk which I employed is 14 inches long and $1\frac{1}{2}$ inch broad. When placed parallel to the needle at the distance of six inches either eastward or westward, the heated pole acquired an ascendancy of deflecting force, agreeable to the first law, for the first minute or a little more: but by continuing the heat, for a minute or two more, the needle would sometimes make an excursion the other way, and it would be difficult to predict at what angle it would settle when the busk had become cooled again. Frequent heating of the poles, however, without retouching the steel, brought the phenomena to a more uniform order.

157. I have already stated (143.) that there appeared to be several ways of explaining the phenomena according to the effects which heat might produce in the magnet, and having now determined the non-interference of electric currents, I next endeavoured to ascertain in what manner each individual pole of the magnet affected the needle. For this purpose the magnet was placed at right angles to the magnetic meridian, having its nearest extremity six inches westward from the pivot of the needle, and sufficiently southward or northward of the magnetic equator of the needle to hold the north, or south end of the latter due west. When the

* It would be useless to detail the whole of the experiments which I have made with a view to test the accuracy of the law, since those which I have detailed appear sufficient to establish it, and that I have met with none that have militated against it. I may just mention that I have made many experiments with the same magnets with their poles placed in the reverse order, so that the marked and unmarked poles of the magnet, were respectively opposite the marked and unmarked poles of the needle; in which case the deflections of the latter were the effects of *repulsion*. The ratio of maximum and minimum deflection was about the same as that developed by the *attractions* in the detailed experiments.

unmarked pole of the magnet was nearest the needle, the north end of the latter was, of course, brought west, and when the *marked* pole of the magnet was nearest, the south end of the needle was brought west. The following tables show the results.

Nineteenth Experiment.

Magnet retouched; in the same horizontal plane as the needle: having its *unmarked* pole nearest to, and six inches westward of, the needle. Observe, the deflections are registered for the north end of the needle. (See fig. 5.)

Deflections.			
Before the magnet was heated	90° W	} Transient loss sustained by heat	} 21°.
Unmarked (nearest) pole heated	69 W		
When the magnet had returned to its original equable temperature	84° W	} Permanent loss sustained by heat	} 6°.
Original deflection . .	90 W		

Twentieth Experiment. (Fig. 5.)

Magnet retouched and placed as in the 19th experiment.

Deflections.			
Before the magnet was heated	90° W	} Transient loss sustained by heat	} 8°.
Marked (remote) pole heated	82 W		
When the magnet had returned to its original equable temperature	86° W	} Permanent loss sustained by heat	} 4°.
Original deflection . .	90 W		

Twenty-first Experiment. (Fig. 6.)

158. The magnet was retouched and placed in the same position as in the two preceding experiments, having its poles in the reverse order, viz., the *marked* pole nearest to the needle, and five inches westward of its pivot.

Deflections.			
Before the magnet was heated	90° E	} Transient loss sustained by heat	} 27°.
Marked (nearest) pole heated	63 E		
When the magnet had returned to its original equable temperature	80° E	} Permanent loss sustained by heat	} 10°.
Original deflection . .	90 E		

Twenty-second Experiment. (Fig. 6.)

The magnet was retouched and placed as in the 21st experiment.

Deflections.			
Before the magnet was heated	90° E	} Transient loss sustained by heat	} 9°.
Unmarked (remote) pole heated	81 E		
When the magnet had returned to its original equable temperature	85° E	} Permanent loss sustained by heat	} 5°.
Original deflection . .	90 E		

159. The results of the 19th, 20th, 21st, and 22d experiments instead of being explanatory of the previously developed phenomena, have some tendency to involve the cause of them in still greater obscurity than before; or, at any rate, they have shown that the cause must be looked for in some other way. By these experiments the heated pole of the magnet invariably lost a much greater quantity of deflecting force than was lost by the unheated one: whereas, by all those experiments in which the magnet was parallel to the needle (155.) the heated pole as invariably gained an ascendancy of deflecting power. Now the latter effect could not happen by the heated pole losing more power than the cool one; unless both moved from the point of heat: therefore the whole mystery seemed to be involved in the following problem. Does the loss of deflecting power as shown by experiments 19, 20, 21, 22, depend upon an *absolute* loss of power in the poles of the magnet, or merely upon a change of their position in the bar, by the agency of caloric?

160. The solution of this problem required a new set of experiments and a magnetic needle of more delicacy than that hitherto used. For this purpose I formed an astatic needle with two sewing needles placed in a stem of dried grass as in fig. 7. The needles were four inches apart with their poles the reverse of each other, and the whole suspended by a delicate fibre of unspun silk, in the interior of a cylindrical glass jar, as seen in the figure. The system, having a slight directive tendency, was properly adjusted to the meridian line of a graduated card which was secured in the jar, beneath the lower needle, which, being the more powerful of the two, was that which gave direction to the system.

161. The 8-inch magnet being newly touched by the exciting horse-shoe, was placed at right angles to the meridian line of the card, and in the same horizontal plane as the lower needle, with its marked pole to the south side of the glass containing the system; and its unmarked pole westward, and so adjusted as to keep the lower needle in the meridian line. In this position, the south end of the needle necessarily pointed to the centre of force, or to the true pole, of the marked end of the magnet. A horizontal plan of the arrangement of the magnet and lower needle is seen in fig. 8.

Twenty-third. Experiment.

162. The lamp was placed under the marked (nearest) extremity of the magnet, and in three minutes the south end of the needle deflected 5° towards the centre of the magnet, showing that the centre of force in the heated extremity had

moved in that direction. The lamp was continued for a few more minutes, but the deflection did not increase accordingly. When the magnet had returned to its original equable temperature, the angle of deflection was about 2° . This latter fact showed that the pole moved back again as the magnet cooled; though it never returned to its original position.

Twenty-fourth Experiment.

163. The magnet was not molested since the last experiment, and the lamp was applied to the remote (unmarked) pole. In ten minutes the south end of the needle had moved over 2° towards the cold extremity of the magnet: that is, it returned to the meridian line: showing that the unheated pole, as decidedly as the heated one, moved *from* the point of heat. When the magnet had returned to its original equable temperature, the needle still remained on the meridian line; or, if it moved at all, the arc was so trifling as not to be perceptible. This singular fact induced me to repeat the experiment with the magnet retouched; because from previous experience I was led to suppose that this circumstance would be the means of giving different results. I consequently retouched the magnet and placed it, as before, with its marked pole to the south end of the needle, as in fig. 9, and so adjusted it that the needle rested directly over the meridian line of the card. The following are the results.

Twenty-fifth Experiment. (Fig. 9.)

164. The lamp was placed under the unmarked (remote) extremity of the magnet. In three minutes the south end of the needle deviated towards the east, or towards the marked extremity of the magnet: and in ten minutes the deflection amounted to about 3° but never proceeded farther, although the lamp was continued under the unmarked end for ten minutes longer; in all 20 minutes. When the magnet had cooled to its original temperature the needle stood 1° eastward, having receded 2° .

165. The first result of this experiment is precisely the same as that in the 24th. experiment, which showed that the centre of force in the cooler end of the magnet moved *from* the point of heat. And the recession of the needle when the magnet cooled was what was expected from the magnet being retouched previously to the application of the lamp.

166. Having now discovered that the magnetic poles are susceptible of transition from one place to another in the metal by the agency of caloric; and also the direction in which the poles move with respect to the calorific point; it

appeared possible that these mutations in the positions of the poles might account for the whole of the other phenomena; although, at first sight, the relaxation of power in experiments 20 and 22 did not seem likely to arise from the pole advancing towards the needle, as must necessarily have been the case according to the results of experiments 24 and 25; unless, indeed, the deflecting power of that pole had absolutely diminished at the same time. But if we take into consideration that the needle was under the influence of *both* poles of the magnet, instead of assuming the deflection to depend on the vicinal pole only, we are still furnished with sufficient data to account for the diminution of deflection even in experiments 20 and 22.

167. *Explanation of the preceding phenomena upon the supposition of a motion of the poles of the magnet, by the agency of calorific matter, as demonstrated by experiments 23, 24, and 25.*

It has been shown by experiments 23, 24, and 25, that the poles of the magnet move *from* the point of heat; and by applying these polar motions to all the experiments represented by figures 1, 2, 3, and 4, we shall find that the deflections of the needle would be such as are there represented. If, for instance, the magnet be so adjusted, in any of those figures, as to permit the needle to rest in the meridian prior to the application of heat; then a slight motion of the magnet northward in figures 1 and 3; or southward in figures 2 and 4, will cause deflections such as are represented by the dotted needles, in these figures respectively. Now, instead of moving the magnets to produce the deflections, let the lamp be applied as shown in the figures; and the motions of the poles by the agency of heat, correspond in direction with the motions of the magnet: and the deflections by one of these means correspond with those accomplished by the other.

168. With respect to the phenomenon exhibited by experiment 23, fig. 5, when the lamp is applied to the unmarked pole of the magnet, as both poles recede *from* the compass needle their power on it necessarily abates; and the earth's magnetism pulls the needle from its east and west position. But when the lamp is applied to the marked, or remote pole of the magnet (experiment 24); both poles would advance towards the needle. Still, however, the experiment shows that the power of the magnet on the needle has abated, which seems contrary to what one would have expected. The phenomenon might be explained, however, by supposing that the heated pole advanced towards the needle in a greater ratio than the cool pole, by which means it would partly neutralize

the effect of the latter on the needle: or by supposing that the total power of the magnet was abated by the heating process: or, which is the most probable of all, by both these changes taking place in the magnet by the agency of caloric.

169. It appears upon the whole, that the polar motions in the magnet are the first grand productions by the agency of caloric; and that, independently of any absolute change in the intensity, the whole of the phenomena which I have described may be traceable to these polar motions: and, perhaps, there may be many other magnetic phenomena, both natural and artificial,* which are attributable to the same *secondary* cause, which is itself an effect of the primitive action of caloric. But as the sun's heat is constantly exerted *between* the magnetic poles of the earth, and not exterior to them, it will be necessary to ascertain in what manner the magnetic poles are affected by placing the point of heat *between* them, before the phenomena can be applied to the explication of terrestrial magnetic mutations. The following experiments were made for this purpose.

Twenty-sixth Experiment.

170. The 8-inch magnet (193) was retouched and placed horizontally at right angles to the magnetic meridian; having a needle arranged at each pole as in fig. 10, directly opposite to the centres of magnetic force. The lamp was placed directly under the centre of the magnet. In one minute both needles began to move; and in five minutes each needle had attained a deflection of 5° , or thereabouts. The deflections were in the directions represented by the dotted needles, showing that both poles of the magnet had moved *outwards*, or, as in the previous experiments, *from* the point of heat.†

* Mr. Barlow's interesting experiments on the magnetic action of heated iron, occasionally gave very extraordinary results which that gentleman could not easily account for. *Barlow's Magnetic Attractions*, second edition, page 142 to 149.

Mr. Christie also, met with some curious anomalies in his valuable experiments on this subject. Phil. Trans. for 1823, 1825, 1826. But whether the facts which I have discovered are calculated to throw any new light on the anomalies which attended these gentlemen's experiments, I am not prepared to say.

† These results, which are so very different to any hitherto made known, cannot be accounted for upon any supposed motions of the neutral plane of the magnet, in the manner which has been attempted to explain some of the interesting phenomena discovered by M. M. Coulomb and Kupffer.

171. This important result in connexion with those results obtained by experiments 23, 24, and 25, appear to develop a certain determinate action which caloric exercises on the poles of a magnet, viz: That the magnetic poles move *from* the point of heat, or in general, that, *the magnetic poles move in the direction of the caloric current.* Should this law become established by future experiments, and that it can be proved experimentally that a current of caloric will move the magnetic poles *laterally* as well as in the direction of their axes, there would be little difficulty in accounting for the revolutions of the terrestrial magnetic poles in their respective latitudes: and I have no doubt, from the results of some experiments that I have made with flat pieces of steel, that the magnetic poles of the earth are susceptible of a *lateral translation* by the direct action of solar heat alone: and that by means of a magnetized steel globe and a spirit lamp I can readily suppose that the revolutions of the terrestrial magnetic poles might be very beautifully imitated. But I have not, at present, any more spare time to devote to this interesting subject. I must, therefore, content myself, till some more favourable opportunity presents itself, with having called the attention of philosophers to this novel mode of investigation, being perfectly aware that there yet remains a rich harvest for those who may venture on the pursuit. In conclusion, I would beg permission to state, that the expansions and contractions of the magnetic axis as shown by experiments 23, 24, 25, and 26, appear to me to afford sufficient data for supposing that the terrestrial magnetic axis suffers similar mutations by the direct action of the sun, and that the phenomena of diurnal variation, and change of intensity on the needle are probably traceable to these *secondary causes.*

Should I be correct in my conjectures, there is not only a cause of *translation* of the terrestrial magnetic poles; but also a cause of *sustentation.* The sun's heat may, possibly, be the *primitive cause* of both: *directly* the cause of *translation*; and, through the intermediate agency of electricity, indirectly the cause of *sustentation.*

P. S. This third memoir was read before the London Electrical Society on the evening of December 4th, 1838; but having withdrawn my name from the list of members of that Society shortly afterwards, I am not aware that it will be printed in its Transactions.

My fourth memoir, which will appear in the October number (No. 21) of these "Annals," was read before the London Electrical Society on the 7th March 1838; therefore these

memoirs are not numbered in the chronological order in which they originally appeared before that Society. The reason of the present arrangement is simply to connect the second with the third, they being portions of the same investigation.

I hope also to have the results of my investigations on the application of electricity to the springing of mines, ready for the next number of the Annals.

W. S

XV. *Note on several new reactions determined by spongy platina, and considerations on the services this substance is required to render science.* By M. FRED. KUHLMANN.*

I have made some researches on the phenomena of nitrification; these have led me to present the theory of these phenomena in a new light. My memoir being too long to read, I shall content myself with leaving it so as not to abuse the time the Academy has wished to grant me. I shall not here enter into any analytical detail of the motives which appear to me likely to modify the actual theories, but I cannot resist the desire of informing the Academy, *viva voce*, of some results to which my experiments on this important question have conducted me; and which, generalized as they deserve to be, will acquire great consequence in the eyes of chemists.

Dœbereiner's beautiful discovery of the property which spongy platina possesses of determining the combination of a mixture of hydrogen and oxygen, has been justly regarded as one of the most precious facts acquired by science for a long time. Every one might foresee the extension that so extraordinary a kind of action would one day take, yet we are astonished at the few facts which have been observed since the date of Dœbereiner's labours, twenty years ago.

The numerous experiments of which I am about to speak, appear to me likely to lead the attention of chemists to a too much neglected question, and which I look upon as the most fruitful in fine results.

1. Ammonia mixed with air, in passing to a temperature of 300° surrounding spongy platina, is decomposed, and the azote which it contains is completely transformed into nitric acid at the expense of the oxygen of the air.

* From the Comptes Rendus, 24th December, 1838. Translated by Mr. J. H. Lang.

2. Cyanogene and air under similar circumstances give rise to the same acid and carbonic acid.

3. Ammonia engaged in any saline combination acts as if it were free.

4. In no case has pure azote been able to be combined with free oxygen, but *every compound of azote under the influence of the spongy platina passes to the state of nitric acid.*

5. Protoxide and deutoxide of azote, hypo-nitric and nitric acid, mixed with a sufficient quantity of hydrogen, are transformed to ammonia by their contact with the platina sponge, and very often without the assistance of heat. The action becomes so energetic that it frequently causes a violent explosion.

All the azote of these oxides or acids passes to the state of ammonia by uniting with hydrogen.

An excess of nitric acid gives a nitrate of ammonia.

6. Cyanogen and hydrogen give ammonia in a hydro-cyanate state.

7. Deutoxide of azote in excess and olefiant gas in becoming hot on the platina sponge, produce, besides water and azote, ammonia united to the hydro-cyanic and carbonic acids.

8. With deutoxide of azote and an excess of alcoholic vapour we obtain under the same circumstances ammonia united with hydro-cyanic and carbonic acids, and accompanied with water, olefiant gas, and a deposit of coal.

9. We have not been able to combine free azote with free hydrogen, but *all the compounds of azote have been transformed into ammonia by free or carburated hydrogen.*

10. In these latter reactions the presence of carbon in combination with azote or hydrogen, produces hydro-cyanic acid.

11. All the gaseous or vaporizable metalloïdes unite without exception with hydrogen under the influence of the spongy platina.

12. The vapours of acetic acid mixed with hydrogen are totally transformed into acetic ether (acetate of ether) and water by the action of the platina sponge at a slightly elevated temperature.

It is a very remarkable fact that by substituting the platinum black for the platina sponge, the energy of action is infinitely less in most cases, contrary to what we might expect. This action is even nullified for producing nitric acid; for producing ammonia it is very feeble and the platina black never enters into incandescence as is the case with the sponge. For the transformation of acetic acid into ether, the action of the platina black is, on the contrary, more lively, and produces it at the ordinary temperature.

It is not to be wondered at that in rendering useful a force which we are not yet well acquainted with, and which a celebrated chemist has designated under the name of catalytic force, we should not be easily able to foresee the result of our attempts.

From the facts stated in this note, and more fully developed in my memoir, I have shown the possibility of obtaining artificially, and at pleasure, nitric acid and, consequently, nitrates, without having recourse to the slow process of nitrication. If under these circumstances the transformation of ammonia into nitric acid, by means of the platina sponge and air, is more economical than our actual process, the time may come when this transformation will form a profitable industry.

We may say with certainty that the knowledge of the facts I have stated may completely set the country at rest on the difficulties or even impossibility of procuring a sufficient quantity of saltpetre in case of a maritime war, and cause the ancient method of providing saltpetre for the necessities of the state to be totally abandoned.

The formation of ammonia with some one of the compounds of azote and oxygen, appears to me likely to engage the attention of philosophers and manufacturers.

It is an important fact obtained by science, that every time azote is found engaged in any combination in contact under the influence of the platina sponge, with an excess of hydrogen or oxygen, it passes to ammonia or nitric; so that in case of failure in producing ammonia we have nitric acid, and vice versa.

Fabrication of nitric acid. The abundant formation of hydro-cyanic acid by the oxides or acids of azote and the carburets of hydrogen, is a fact not to be neglected in so scientific a question as the production of cyanurets, and particularly Prussian blue.

The transformation of vinegar into acetic ether, assures us that divided platina promises also, in perhaps a little longer time, applications equally important in the arts concerning organic matter.

It is well known that acetic ether is easily transformed into alcohol by the action of the alkalies and water; but alcohol has hitherto only been obtained by the fermentation of sugar; its preparation from vinegar, for which the sources of production are so numerous, shows the possibility of one day making alcohol by much less expensive means, and doubtless alcohol will make no exception.

Whatever it may be, the facts cited, the only ones which I have been able to mention in my memoir, without wandering

too far from my subject, are sufficient to show, even with evidence, the important field reserved for divided platina.

The mean of action which produces such numerous combinations, such various transformations, will give rise to new products; it will become to the chemist as useful, and have an application almost as general, as heat and electricity.* We shall very often be able to find in the sponge or black of platina a source of action which no other known agent will be able to supply: it is particularly when it operates on changeable bodies, at an elevated temperature, that divided platina will be of the greatest service.

In another work, which I shall have the honour to submit to the Academy, I shall complete the account of all the results I have obtained by means of the platina sponge; mentioning the new facts I may be able to observe.

Note on Lightning Conductors.

The important epoch which has now arrived in practical Electricity, by our Government appointing a committee of naval officers to enquire into the most effectual mode of parrying off the effects of lightning in her Majesty's fleet, demands the most assiduous attention to the subject, and the unpromising opinion of every electrician in these realms, respecting the best mode of protecting vessels from this terrible element. We have heard that the plan proposed by Mr. Snow Harris has been approved of by the committee. This plan consists of a strip of copper let into the whole length of each mast: those of the lower masts passing through the keel of the vessel; and that of the mizen mast passing through the after powder magazine. We shall be very happy to give the views of any electrician on this most momentous topic; which, if not directed by the ablest electrical skill, will not only involve the science of this country in the most degrading position, but will put in unnecessary, nay, wanton, jeopardy, our brave naval officers and men, who have so long been the bulwark and glory of our country.

EDIT.

* We are of opinion that the spongy platina operates in the capacity of a voltaic battery, having a multitude of polar points: and that electricity is the *real* operating force in all these experiments, which is perfectly agreeable with our views respecting the chemical action of simple metals with fluid menstua.

The original experiments of Dœbereiner with spongy platina on the gases, we have always looked upon as of a similar character, and their results produced by electric currents. See Vol. I, p. 11, 23, of these Annals. EDIT.

THE ANNALS
OF
*ELECTRICITY, MAGNETISM,
AND CHEMISTRY;*

AND
Guardian of Experimental Science.

OCTOBER, 1839.

XVI. *Experimental and Theoretical Researches in Electricity, Magnetism, &c. Fourth Memoir.* By WILLIAM STURGEON, Lecturer on Experimental Philosophy at the Honourable East India Company's Military Seminary, Addiscombe, &c.

Addressed to the British Association for the Promotion of Science.
Birmingham Meeting, Sept. 8th, 1839.

On Marine Lightning Conductors.

The proper adaptation of lightning conductors to shipping being a subject of vital importance to every maritime community, both British and Foreign, the author has been induced, respectfully, to submit this memoir to the consideration of the following scientific bodies.

British Dominions. The Royal Societies of London and Edinburgh.—The Royal Irish Academy.—The Cambridge University Philosophical Society.—The Philosophical Societies of Newcastle-upon-Tyne, Bristol, Manchester, Hull, Liverpool, and Yorkshire.

Foreign. The Royal Society of Sciences at Copenhagen.—The Royal Academy of Sciences at Paris.—The Royal Academy of Sciences at Thoulouse.—The Royal Academy of Sciences at Berlin.—The Italian Society of Sciences at Modena.—The Royal Academy of Sciences at Turin.—The Royal Academy of Sciences at Brussels.—The Royal Institute of Amsterdam.—The Royal Academy of Sciences at Lisbon.—The Imperial Academy of Sciences at St. Petersburg.—The Royal Academy of Sciences at Stockholm.—The Royal Society of Sciences at Dronthem.—The American Philosophical Society at Philadelphia.—The New York Philosophical Society.—The American Academy of Sciences at Boston.

172. In addressing this memoir to the British Association for the Promotion of Science, I am far from supposing that there can exist a necessity for entering into a routine of details in order that the subject may appear sufficiently important for its consideration. It is one well known to be, at all times, interesting to the philosopher, and, at the present moment,
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when the British fleets are about to be furnished with lightning conductors, it has become a subject of high national importance, and demands the most profound consideration of every experienced electrician in the land, in order that the most efficient and economical plan may be adopted and carried into effect.

173. Without any pretensions to a knowledge of the motives for not making so momentous and unsettled a topic open to fair scientific discussion, there can be no doubt of the scantiness of publicity being the sole cause of so *few plans** being brought before the notice of the Committee appointed to enquire into the best means of protecting shipping from the effects of lightning. I am not aware of the objections of the Committee to the plan proposed by Mr. Martyn Roberts, nor of the reasons for giving a preference to that so long proposed by Mr. W. S. Harris: but as the House of Commons has been officially informed that the Committee have determined in favour of the conductors of the latter gentleman, the subject is now open to impartial discussion: and as it is possible that, unless some timely council interpose, the recommendations of the Committee may induce the Admiralty to adopt Mr. Harris's conductors, and, without further enquiry respecting the efficiency, or inefficiency, in parrying the effects of lightning, give immediate orders for their application to Her Majesty's fleets, no time ought to be lost in placing Mr. Harris's system of conductors in a *proper light* before the Government and maritime community of these realms.

174. There certainly cannot be a finer opportunity offered to the British Association for exercising its almost boundless influence in eliciting important philosophical truths, and extending the benefits of experimental and practical science, than that now presented by this momentous topic; nor can electricians of every country have a more favourable opportunity of becoming benefactors to mankind, than by giving this universally important subject their profoundest contemplation; and, as soon as possible, making known the results of their

* Only two plans were brought before the Committee, the one by Mr. Martyn Roberts, and the other by Mr. W. S. Harris. The plan of the former gentleman consists of a rope of metallic wire one end of which is hoisted to the mast head, and the other thrown over the side of the vessel. See *Annals of Electricity, &c.*, Vol. I. p. 468, Vol. II. p. 241.

The plan of the latter gentleman consists of strips of sheet copper, one to each mast, let into a groove the whole length of the after-side, and passing through the keel of the vessel, and that belonging to the mizen mast passes either through or close to the after powder magazine.

experimental investigations in this branch of electricity, and their uncompromising opinions on Mr. Harris's system of conductors, on which so much deep interest is now involved: or by proposing any better plan of protection to the thousands of lives and millions of property continually exposed to the most formidable and destructive of nature's elements.

175. The present memoir contains:—*First*, an examination of those experiments which Mr. Harris has exhibited on various occasions, and at various places, in illustration of the supposed superiority of his system of conductors. *Second*, an examination of the *observed* effects produced on shipping by lightning. *Third*, a statement of electrical phenomena similar to those effects, and probably productive of others no less destructive. *Fourth*, a comparison of the *observed* effects of lightning and the *probable* effects which lightning would produce by the adoption of Mr. Harris's system of conductors. *Fifth*, a description of a new system of conductors for the protection of shipping from lightning.

Examination of Mr. W. Snow Harris's experiments.

176. *Experiment 1.* Let *j*, fig. 1, Plate IV., represent the electrical jar on board the Caledonia; *c* the Louisa cutter, with a strip of sheet copper let into the after side of her mast from top to bottom, and continued, through her keelson, to the sea, as represented by the strong black line. By means of a chain *ooo*, the lower extremity of this conductor is brought into metallic connexion with a loaded brass howitzer in the boat B, moored at some distance from the cutter. A wire *www*, proceeds from the outside of the jar to the boat, and terminates above the vent of the howitzer, leaving a small interruption for the introduction of a priming of *detonating powder*. By means of another wire *w'w'*, one end of which is fastened to the conductor at the mast head of the cutter, the jar is discharged in the direction indicated by the arrows; and the spark which passes between the howitzer and the end of the wire *www*, ignites the *detonating powder*, and the piece thus becomes discharged: whilst a portion of *common gunpowder*, placed in contact with the *uninterrupted* conductor at *p* in the cutter, is *not exploded*.

Experiment 2. The only difference in the arrangement from that in the last experiment, is simply that of interrupting the metallic connexion between the lower extremity of the cutter's conductor, and the howitzer in the boat B, and permitting the sea to form part of the circuit. The effects were the same as in the first experiment.

Experiment 3. The arrangement of apparatus the same as before, with the exception of an interruption in the cutter's conductor at *p* by passing a saw through it. No gunpowder nor *detonating* powder at *p*. The howitzer was discharged as in the preceding experiments.

177. These three experiments were made in Plymouth harbour, in September, 1822, "in presence of the Navy Board, Sir Alexander Cochrane, Commissioner Schield, several Captains of the Navy, and the principal Officers of the Dockyard," with the intention of convincing them of the efficacy and superiority of Mr. Harris's conductors: although it must be obvious to every electrician that these experiments had no peculiar bearing on any peculiar plan of conductors whatever: for they are just as applicable to the illustration of the efficacy of the *present chain conductors* or to the *metallic rope* proposed by Mr. Roberts, as to those proposed by Mr. Harris.

178. The first experiment showed that copper is a conductor of electricity: that *detonating* powder can be ignited by an electric spark: and that *common gunpowder* will *not* ignite by the application of cold copper, though a feeble momentary electric current traverses the metal at the same time. The second experiment, in addition to the facts shown by the first, shows that sea water is a conductor of electricity. The third experiment, in addition to the facts shown by the first and second, shows that a *slight* interruption in the metallic part of the circuit did not prevent a spark, from the jar employed, from igniting *detonating* powder at another *slight* interruption of that circuit.

179. These, I believe, are the only facts which Mr. Harris's experiments, exhibited in presence of the Navy Board, are calculated to illustrate; and as they prove nothing novel, nor anything peculiar to that gentleman's system of conductors, they not only fall short of their object, but are perfectly inapplicable to the great question at issue.

180. There are, however, circumstances connected with these experiments, which, though not noticed by Mr. Harris, would have been quite as interesting to the Navy Board as the results which they witnessed. Mr. Harris could easily have shown the officers of the Navy Board, that a moderate electrical discharge would render a copper conductor *red hot*, and that gunpowder in contact with this electro-heated metal would explode as decidedly as if heated by any other means: and it would have been no more than a just inference to have added, that, a lightning conductor may be sufficiently heated to fire gunpowder, by a discharge of lightning from the clouds. It would have

been well, also, to have shown that *detonating* powder would be as sure to ignite by the spark at the interruption made by the saw in the cutter's conductor, as by a spark at the howitzer. From the first of these facts, those officers might probably have inferred that a lightning conductor passing through the powder magazine would be the most dangerous appendage in the ship: and from the latter they would have implied that the most trifling interruption in the conductor, from accident or otherwise, might be productive of serious evils.*

181. In a lecture delivered before the members of the British Association at the Liverpool meeting, Mr. Harris brought forward the following additional experiment in illustration of the advantages of his system of lightning conductors.

Experiment 4. In fig. 2, *ab* and *cd* represent the two halves of a long tapering round pole, cut along its axis from end to end, having the flat sides of both halves towards the spectator. In the axis of *ab* are embedded three pieces of metallic wire *an*, *nm*, and *mb*. At the interruptions *n* and *m*, are small cavities in the wood for the lodgment of gunpowder and *detonating* powder; and when charged with these combustibles the flat side of *cd* is placed against the flat side of *ab*, the two being slightly kept together by a suitable contrivance. The outer surface of this compound piece is furnished with a strip of tinfoil which reaches from top to bottom: and is then represented as a perfect model of a ship's mast furnished with Mr. Harris's conductors.

When a jar is discharged from the top to the bottom of this mast, the electric fluid traverses the tinfoil on the outside, and the mast is protected. This done, a portion of the tinfoil conductor, reaching from above the opening at *n*, down to below the other opening at *m*, is taken away: and the next discharge of the jar necessarily passes through the axial wires, and ignites the powder in the two cavities *n* and *m*; and the explosive force of the gunpowder blows off the moveable half *cd* of the mast. This effect was held forth as an exemplification of the effects of lightning on a ship's mast.

182. Whatever may be the merits or demerits of Mr. Harris's system of conductors, it is very remarkable, that the experiments exhibited by that gentleman in its favour, before the Navy Board, at Plymouth, in 1822, and repeated in his lectures before the British Association, at Liverpool, and other places, happen to have no bearing whatever on the subject.

* I believe, however, that Mr. Harris has taken every precaution to prevent interruptions in the conductors which are applied to the masts: but at the steps of the masts, and between the copper bands and bolts in the keelson, it will always be difficult to keep metallic contact so as to prevent explosions.

Those experiments are no more illustrative of the efficacy of Mr. Harris's system than of any other system ever yet offered to public notice: and as I am not aware that any other experiments have been attempted for demonstration, I have no means of knowing how that gentleman has been led to neglect some of those considerations, which, next to the selection of the conducting material, are certainly the most important in the equipment of shipping with lightning conductors.

Examination of the observed effects produced on shipping by lightning.

183. A general outline of the effects of lightning on shipping cannot be better portrayed than by the following description of those produced on the *Rodney*, on the 7th of December last.

FATAL EFFECTS OF LIGHTNING ON H.M.S. RODNEY, IN THE MEDITERRANEAN.

H.M.S. *RODNEY*, 7th December, 1838, was 8 or 10 leagues to the eastward of Cape Passaro, blowing strong with squally overcast and rainy weather, under close reefed fore and mizen and treble reefed main top sail. At 9 A.M., in a heavy squall attended with hail and hard rain, the ship was struck with lightning. Three or four flashes of angry looking forked lightning had been seen before in the northern horizon, not attaining an altitude of apparently more than 10 or 15 degrees, and the writer thinks it here necessary to mention that this statement is drawn up entirely from his own ideas, sensations, and observations on the occasion, and not from any hearsay reports, because many on board saw and heard things differently to himself. He was standing with Captain Parker close to the wheel at the time. No very particularly vivid flash or unusually great glare of light was observed at the time, although people are commonly blinded for some seconds on such occasions, but the first thing heard was the burst or explosion, which was louder than one of the ship's 32 pounders, but not exactly like it, it being sharper and more piercing, something like the bursting of a bomb shell; and then the iron hoops of the main mast came rattling down the mast, being burst asunder. The captain and the writer looked at each other in silent astonishment, then forward and aloft, when the hoops were seen tumbling down the main mast, and splinters flying about in all directions and to leeward to an immense extent. The main topgallant mast was shivered to

atoms, leaving the main topgallant yard across the cap, and the royal pole from the hounds of the topgallant rigging to the truck perfect, which was left sticking up above the topgallant yard, having slipped itself on the upper part of the topmast rigging. In perhaps about a minute or so, flames were seen issuing from the bunt of the topsail yard, the electric fluid having ignited the paunch mat and gear thereabouts; it burnt some minutes before it could be extinguished by wet swabs, water, &c., &c.; fortunately from the heavy rain every thing was perfectly saturated, or no one knows where the mischief would have ended. The electric fluid was seen to pass from the mast about 7 feet above the deck over the star-board hammock netting to leeward (right over a gun) like a ball of fire. Luckily all the people on deck had been ordered to take shelter out of the rain, consequently nobody was near the main mast at the time, and nobody suffered excepting the unfortunate men who were at the mast head.

Progressive course of the Electric Fluid.—The vane staff which is 6 feet long with a copper spindle (on which the vane traverses) of about 10 inches in length, surmounted by a gilt wooden ball, the size of an orange, shows its first effect (the ball and spindle were never seen after the shock), being split but not broken, and one side of it blackened; the copper binder round the truck was burst asunder, a small piece broken out of the truck, and one of the metal sheaves for signal halyards slightly fused. From this, after leaving the royal pole uninjured, it appears to have passed inside the copper funnel for topgallant rigging and iron hoop of the hounds of the mast, shivering the topgallant mast to atoms, from thence to the topmast cap, not a piece having been seen the size of a common walking cane, and the sea was literally covered with its splinters to a considerable extent; its marks are now lost for many feet, notwithstanding the shock about this spot must have been most terrific, as it was in the topmast crosstrees where the poor fellows who suffered were at the time, and also the heel of the topgallant mast (which was not at all injured below the cap) was forced upwards into the cap, the fid being raised about 8 inches above the trussel trees with such force that the top burton block strop was carried away in trying to bouse it down again, and after all we were obliged to cut it out, not being able to clear it in any other way. Its next appearance is on the main topmast, 10 feet above the cap, seemingly attracted by the iron-bound tye blocks and iron hoops on the topsail yard (being under a treble reefed topsail), from whence it rent an immense splinter out of the mast down to the lower cap, going nearly into the

core of the mast and set fire to the tarry and greasy gear about the bunt of the topsail yard, after taking this large splinter of nearly one quarter of the substance of the mast away. Its next positive mark is on the starboard lower trussel tree, the lower cap, head of lower mast, and heel of topmast (both iron hooped), having escaped unhurt. It shook and blackened the trussel tree, rendering it unserviceable, and then must have entered the main mast, spreading and passing down both sides, bursting 13 of the large iron hoops in its course, and knocking out pieces of the side trees and main stick in several places, and escaped from the mast in the shape of a fire ball, 7 feet above the deck, and was seen to go over the starboard (leeward) netting right over the gun abreast of the main mast rending the hammock cloth in several places, carrying away one rattlin and stranding another; its exit although fiery in appearance was harmless in effect, merely injuring the cloth over a space of about a foot, and breaking the two rattlins, when it was seen to strike the water a short distance from the ship.

Effects of the Electric fluid in its course.—Knocked overboard (at least they were never picked up or seen) the gilt ball, copper spindle, and calico vane from the top of the vane staff—split the vane staff—broke the copper binder round the truck—broke a piece out of truck and slightly fused one of the metal sheaves for signal halyards—cleared away the whole of the main topgallant mast from the hounds of topgallant rigging to the topmast cap, not leaving a fragment aloft. Four men who had been sent aloft to unbend topgallant gear and prepare for sending the yard down, were in the crosstrees at the time; John Rowe was struck dead as he was moving from the weather to the lee side of the mast for shelter from the rain, he was just on the aft side of the mast at the moment and fell astride the after crosstree, where he was held by some ropes falling round him—he never spoke. Thomas Hollingsworth was standing on the after shroud of topgallant rigging to leeward of the mast and holding on by the after crosstree—he was so seriously injured as to be sent down in a chair and died in 7 hours after. Hugh Wilson was standing on the foremost shroud of topgallant rigging, holding on by foremost crosstree and close to Hollingsworth—he states that the shock threw him forward and Hollingsworth aft, he was only slightly hurt, and only two or three days in the doctor's list; the other man Charles Prynne was to windward, standing on the crosstree, holding on by the foremost shroud of topgallant rigging and received so slight a shock that he did not even apply to the doctor. Wilson heard no thunder. The first

named two men had every stitch of clothes burnt from their bodies, excepting just the wrist bands and lower parts of the trousers which was left about the wrists and ankles. They presented a shocking spectacle, their bodies discoloured and hair singed from their persons. The next place is a large splinter out of the main topmast, from ten feet above down to the cap, setting fire to the gear about the topsail yard and then commences its destructive force about the main mast, first of all giving a severe shake to the starboard lower trussel tree; it is hardly possible to give a description of its effects on the main mast, the mast should be seen fully to understand it, but some idea may be formed when it is stated that out of 28 large iron hoops 5 inches wide and half an inch thick between the deck and trussel trees, 13 were burst asunder and that for a space of 53 feet its ravaging effects can be traced the whole way, and the spot whence it made its final escape is several inches deep in the mast—on the starboard side a large piece of the mast is broken out (6 inches deep) from the third to the sixth hoop above the deck, and from the eighth to the ninth hoops. The cheek or side tree several feet of the lower part gone altogether and the other part nearly shook all to pieces. The larboard side—ekin piece gone from the sixth to the eleventh hoop and the mast burst out from the ninth to the eleventh, and from the thirteenth to the fifteenth, and the cheek very much shook. The hoops carried away were mostly the clasp hoops of side trees, but some of the body hoops were also burst asunder, and strange to say the awning hoop on which the main trysail mast steps and mizen stays reeve, lost one of its fore-locks, notwithstanding a piece of copper had been nailed over the clasp part—the forelock which was driven downwards was gone with a piece of copper and never seen, while the one which drives upwards was left in its place and held the hoop together. There were eighteen body hoops between the deck and trussel trees, and ten clasp hoops round side trees—four of the body hoops below side trees were broken, none of the hoops on the head of lower mast or on the heel of the topmast were touched.

Several men assert that balls of fire were running about the lower deck, and that they ran after them to throw them overboard; this seems strange, but if so, and it is hardly possible several could be deceived, it could be nothing more than flashes or rather sparks passing down the different hatchways after the explosion and less active than in the first descent, at all events it is certain there was a strong sulphurous smell below, particularly in the pump well, and sparks seen by many of the officers. It is remarkable that the

electric fluid seems to have jumped from metal to metal—first the copper spindle, then the copper funnel of topgallant rigging, and iron hoop round the mast to the head of the top-mast, from thence to the iron bound blocks and hoops on the top-sail yard to the main cap, and then to the lower trussel-trees, taking all the hoops downwards, passing over a gun into the sea.

The mast has since been taken to pieces at the naval yard at Malta, and its interior shows no defect, in fact, not the slightest injury appears about the mast, except what was exteriorly displayed; it is marked in some places, even on the spindle (centre piece) as if a train of powder had been flashed on it, but nothing more.

The outside crippled state of the mast led us to fear the worst and to secure it as effectually as possible for carrying sail. This was done by getting the spare fore top-mast up and down, filling up with studding sail booms and clapping the iron fishes over the worst part, the main top-mast being lowered sufficiently to bring its wound below the cap. All was well wounded together, and by degrees we felt confidence in the strength of the mast, so that at last we got a fore-top gallant mast, fitted on it and carried the main-sail, treble reefed topsail, topgallant sail and royal, working to windward for three or four days.

G. B. H.

Magnetism and Electricity.—(Extract of a letter from Malta)—After the mainmast of H.M.S. *Rodney* was struck by lightning during her late passage from Athens to this place, the broken hoops surrounding it, were all found to be magnetised in the same uniformity of direction as if they had been operated on in one direction by the galvanic helix. Thus in a hoop broken in two athwartships (speaking with reference to the ship's head) the larboard end of the foremost portion was a south and its starboard end a north pole: the end of the aftermost portion in contact with the south pole of the foremost portion being consequently a north pole, and the other end thereof a south, and so uniformly with all the other hoops at whatever part they were; similar poles in each hoop always pointing in similar directions in the circumference of the respective circles.

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184. There are five remarkable facts stated in the above account from the *Rodney*, which will be occasionally alluded to in this discussion: viz. The bursting of the metallic hoops of the main mast, copper binder of the truck, &c.—The fusion of the metallic sheave.—Wilson and Holingsworth being thrown in opposite directions.—The flashes or sparks seen

below deck:—and, The manner in which the broken hoops were magnetised.

185. The two following are cases in which ships, furnished with Mr. Harris's conductors, are supposed to be struck with lightning.

Extract from a Report on the Lightning Conductors of H.M.S. Beagle. 1831-6

"Previous to leaving England in 1831, the Beagle was fitted with the permanent lightning conductors invented by Mr. W. Snow Harris, F.R.S.

"During the five years occupied in her voyage she was frequently exposed to lightning, but never received the slightest damage; although supposed to have been struck on at least two occasions. At each of these times, at the instant of a vivid flash of lightning, accompanied by a crashing peal of thunder, a hissing sound was heard distinctly on the masts, and a strange, though very slightly tremulous, motion in the ship herself, indicated that something unusual had happened, &c.

(Signed) "ROBERT FITZ-ROY,
"Late Captain of H.M.S. Beagle."

Extract from a Copy of a Letter from Captain Turner, late of H.M.S. Dryad.

"H.M.S. Dryad, Sierra Leone, Feb. 13th, 1831.

"My Dear Sir,

"I write to inform you that we have had a trial of your conductors, and most excellent things they are. During our last cruise we had a great deal of lightning, but in one afternoon in particular, we had a tornado, which is always attended with a great deal of thunder and lightning; whilst standing on the quarter deck during one of the flashes, I distinctly saw the lightning run down the conductor on the foremast, and the officer of the fore-castle came and told me he heard a whizzing noise resembling water boiling. All the men that were there heard it also; a short time afterwards several of the officers were standing abaft, and saw it during another flash go down the mizen mast with the same whizzing noise. It may be necessary to tell you that lightning on this coast appears to remain longer in the air than any other place I was ever at.

"I am &c. &c.
"WILLIAM TURNER."

186. Mr. Harris observes, that, there is "a remarkable agreement" in these two cases, especially in "the hissing sound &c." On this phenomenon I shall remark, that the hissing sound proceeds from no other parts of a conductor than those which deliver the fluid into the air or other inferior conducting medium; for no such noise is ever produced by the fluid *entering* a metallic conductor. Hence it is obvious that if these hissing sounds proceeded from Mr. Harris's conductors, they were occasioned by the electric fluid rushing from the sharp edges of them, and from numberless asperities on the surface of the metal. To those accustomed to making electric-kite experiments during thunder storms, the hissing noise must be a familiar phenomenon. It proceeds from the broken parts of the metallic strand and other asperities of the string, which are discharging fluid from *upper* to *lower* strata of the air, or to the ground; and not from any flash of lightning striking either the kite or its string. Hence the hissing noise heard on board the *Beagle* and *Dryad* was no indication of either of the vessels receiving a flash of lightning at the time; but rather that their conductors, and other parts of the rigging, were discharging into the air about the *lower* part of the masts, a quantity of fluid communicated to their *upper* parts by a *wave* of the electric fluid, produced by a neighbouring flash of lightning. There is certainly something remarkable in the appearance of lightning running down the masts of the *Dryad*, which I shall endeavour to explain under another head (208, 209, 210, 211, 212, 213, 214); but whether they emanated from direct discharges of lightning on the masts or otherwise, they could be no very welcome visitors in the powder magazine.

187. Mr. Harris has stated that "from about 100 cases, the particulars of which have been ascertained, it appears that about one half of the ships struck by lightning, are struck in the main mast: one quarter on the fore mast; one twentieth on the mizen mast and not more than one in a hundred on the bowsprit. About one ship in six is set on fire in some part of the masts, sails, or rigging. In these 100 cases there are destroyed or damaged 93 lower masts, principally line of battle ships and frigates, 83 top masts, and 60 topgallant masts."*

188. By looking over the particulars of 174 cases,† which Mr. Harris has collected, I find only 44 in which the top-

* Harris's "state of the question relating to the protection of the British navy from lightning."

† In several of these cases the particulars of the damage is not specified.

gallant masts appear to have been injured: and as out of these 44 cases there are 13 in which the topgallant masts were lost, broken, or damaged, accidents probably occasioned by the mere falling of those masts when the others below them were struck, there would appear to be only about 31 cases out of the 174 in which the topgallant masts have been absolutely struck by the lightning. It is probable, indeed, that the proportion is even less than this; because of these 31 cases there are 15 in which the topgallant masts were shivered only, a species of damage which, if occurring near to the heel of the masts, might easily arise from lightning striking the ship no higher than the topmast head. Lightning striking the topsail yard arm, when that sail is set, or the crosstrees at other times, would be very likely to damage the lower part of the topgallant mast.

189. An accurate statement of the *highest point* which lightning has struck each vessel, and the cases in which yard arms have been struck, would afford important data; for showing the necessary height to which conductors ought to ascend in the rigging, and the comparatively uselessness of continuing them higher. Also, how far it would be advisable to protect the yard arms for the purpose of preventing lightning entering the vessel by their instrumentality.

Electrical phenomena similar to those produced by lightning.

190. The figure, dimensions, and character of the metal being determined on, the next most important consideration in the application of lightning conductors to shipping, are their most eligible situations, both as respects the working of the sails and avoiding the probable disasters by lightning entering the rigging and hull of the vessel; and to determine which, it becomes necessary to study the effects of both the *direct* and the *lateral* discharges.

191. By the *direct* or *primitive* discharge, we know that men can be killed, inflammable bodies ignited, and metals even fused. Hence our first object should be to keep the *direct* discharge as clear of the ship as possible; especially from the masts, deck, and hull; and as it is possible that the most spacious conductor that can conveniently be applied to a ship may be rendered sufficiently hot, by a flash of lightning, to ignite gunpowder,* we ought never to be induced, under

* Instance the fusion of the metal sheave of a block on board the Rodney (183, 184). Some few years ago the brig, Jane, from New York to Liverpool, had some part of her *chain conductor* fused by a flash of lightning; the lower part of it fell overboard. It is said

any circumstances whatever, to lead a lightning conductor through, or near to, the powder magazine, at the risk of blowing up the ship.

192. The *lateral discharges* are of three distinct kinds, which, for convenience of reference, I will denote by the *first*, *second*, and *third* kind.

193. The first kind of lateral discharge, was, by Priestly, called the *lateral explosion*. It takes place at every interruption of a metallic circuit, or, wherever the electric fluid is exhibited in the shape of a spark or sudden flash of light in the common atmosphere or other inferior conducting medium. By this kind of lateral discharge, the air is suddenly displaced: loose bodies are scattered and thrown from the axis of the circuit, and solid inferior conducting bodies are shattered or torn to pieces: *waves* of electricity are also produced in the neighbouring atmosphere and the bodies within it, by this kind of lateral discharge. Hence the probability of ships' masts and other masses of wood being shivered, split, &c.; their hoops burst asunder, and other similar effects being produced from the same cause: even men may be killed by these lateral forces (183, 184).*

194. The *second* kind of lateral discharge is a species of radiation, of the electric matter, from the surface of good conductors carrying a *direct* or primitive discharge. It takes place most copiously from the edges of strips of metal, or from the surfaces of ragged or asperous wires; and to a greater extent in rare than in dense air. Hence sharp edged strips of metal carrying a heavy flash of lightning would necessarily discharge a great quantity of fluid from both edges to the neighbouring objects or into the air.

195. The *third* kind of lateral discharge is a displacement of the electric fluid natural to those bodies which are vicinal to a continuous conductor carrying the primitive discharge. The following experiment will serve as an illustration.

Experiment: Let the Leyden jar *j* fig. 3, Plate IV, be discharged through the good conducting metallic rod *c c*,

that the lightning rod passing through the Nelson monument, at Edinburgh, became so hot, by a flash of lightning, that it could not be touched by the hand, by the first person who visited it afterwards. Allowing only a few minutes to have elapsed between the flash and the person entering the monument, the probability would be that the conductor had been rendered red-hot by the lightning.

* The *principal* discharge obviously passed through and over the surface of Hollingsworth's body; whilst Wilson was probably hurt, and thrown in the opposite direction, by the lateral explosion.

which stands on the same metallic plate as the jar is placed on. At the same moment a spark will appear at the opening *o*, between the insulated metallic body *B* and the conductor *c c*. This is a lateral discharge of the *third kind*.

196. If, instead of the metallic body *B*, the knuckle were to be presented to the conducting rod *c c*, a pungent spark would be felt at every discharge of the jar through the circuit represented by the figure.

197. If, instead of discharging the jar through the conductor *c c*, it were to be discharged by a direct application of the discharging rod to the coating or metallic plate on which the jar stands, as represented by fig. 4; the lateral discharge would still occur at *o* between the body *B* and the conducting rod *c c*.

198. By this kind of lateral discharge a dense spark may be produced when the bodies *B* and *c c*, are half an inch asunder, though the jar employed be only of the capacity of a quart. Chemical decompositions are easily performed by this lateral discharge, even with the small jar I have just mentioned, and every other class of electric phenomena may be exhibited by it, when the apparatus are sufficiently large, as decidedly as by the primitive discharge itself.

199. The effect is much increased by connecting the body *B* with the ground; and diminished, to a certain extent, by connecting the outside of the jar with the ground. I have produced the spark between *c c* and the body *B*, when placed 50 feet from the *direct* discharge. The arrangement is shown in fig. 5, where *c o* is connected with the outside of the jar by a copper wire 50 feet in length.

200. In fig. 6, *c c* may represent the conductor of a ship's mast, connected with the iron knee *k* by means of the copper strap *s*, as proposed by Mr. Harris. Then a discharge from the jar *j* through the conductor *c c* would imitate a flash of lightning striking a similar conductor in a mast; and it will be found that this arrangement is as productive of the lateral discharge at *o* between the body *B* and the iron knee *k*, as any other arrangement whatever; and as similar lateral discharges would take place from any part of the copper strap *s*, or from any metallic appendage or branch of the conductor, it is obvious that men who happened to be near to any of these conductors, straps, knees, &c. would experience all the effects of these lateral discharges.

201. When the body *B* is connected with the conductor *c c*, by a wire *w w*, 20 feet long, as represented in fig. 7, only a very short lateral spark is obtained at *o* between the conductor and body *B*; but as the wire is lengthened the lateral spark becomes longer and denser. I find, also, that the

lateral spark is increased by an increase in the quantity of metal between the body B and the jar, without lengthening the conducting distance.

202. I have united, by metal, the outside of the jar *j* and conductor *cc* with a metallic pump, which descends into a deep well copiously supplied with water, in order that the conductor *cc* might be as uninsulated as I could possibly make it; still, however, the lateral spark appeared between the conducting rod *cc* and the body B, whether the latter was metallically united with the same pump, or with a kitchen range at some distance from it. I have made a number of experiments of this kind, and have varied the arrangement in many ways, and from the results which have been obtained, I am fully of opinion that this kind of lateral discharge will always take place when the vicinal bodies are sufficiently capacious, and near to the principal conductor which carries the primitive discharge, or to any of its metallic appendages.

203. Such, in fact, appears to be the case when only the small jar, already mentioned, is used: and as the extent of electro-displacement, in vicinal bodies, depends upon the magnitude and intensity of the primitive discharge or *main stroke*;* the electro-displacement and consequent lateral discharge occasioned by a heavy flash of lightning striking a conductor which passes through the body of a ship, must necessarily be exceedingly dangerous; for although this kind of lateral discharge is never so powerful as the primitive discharge, nor so great amongst inferior conductors as amongst those which conduct freely; the *magnitude* and *intensity* of a flash of lightning being infinitely greater than any thing which can be produced artificially, even by the most stupendous apparatus that can be employed; the effects of lateral discharges from lightning would also be proportionally greater than any ever produced, or possible to produce, experimentally: and their consequences always to be dreaded. The "balls of fire" seen "running about the deck" of the Rodney, with the flashes and sparks (183, 184), were more likely to be a series of lateral discharges of this kind, or by *waves* from the first kind (193), amongst the articles below, than any part of the lightning which struck the ship. Electrical waves produced by a discharge through a conductor situated close to the powder magazine would produce intense sparks amongst the powder barrels, whose metallic linings and hoops would reciprocally exchange them.

* *Main stroke* is the name given to this phenomenon, by Viscount Mahon, who studied this kind of lateral discharge very extensively.

A comparison of the observed effects of Lightning, and the probable effects which Lightning would produce by the application of Mr. Harris's system of conductors to shipping.

204. Were there no other data than those afforded by the *fusion* of the metallic sheave, belonging to the Rodney (183, 184): and the fusion of the chain conductor belonging to the brig Jane (191 note), we should have ample demonstration of the super-eminent calorific powers which lightning exercises on metallic bodies of considerable dimensions: and of the probable effects producible on masses of still greater magnitude, by the same agency. These specimens of electric action when exhibited on the grand scale of nature, and which mock every human effort at competition, ought to be regarded as monitors of inestimable value, prominently and opportunely placed before us, as if commissioned by an all-merciful Providence, to warn us of the imminent danger, or certain destruction, to which thousands of mortals may be exposed by the misplacing of lightning conductors in ships containing combustible materials. Let us, then, avail ourselves of these well authenticated and most important facts, and endeavour to profit by the inestimable examples which they afford for our instruction and guidance. The impressions which these facts convey to the mind are too forcible and too definite to be easily misunderstood: they clearly imply that either of the discharges of lightning which struck the Rodney, or the Jane, would have been powerful enough to have rendered even the thickest part of Mr. Harris's conductors *sufficiently hot to ignite gunpowder*; and that a similar discharge of lightning striking the mizen mast conductor, should it pass through the powder magazine, would probably be the means of the ship being blown to atoms. Hence it becomes obvious, that the calorific effects alone, by a *direct* discharge of lightning, would be sufficient to put in the most imminent jeopardy every vessel which is furnished with conductors that pass through, or near to, the powder magazine.

205. It appears from Mr. Harris's list of cases (187), that the lower masts are more frequently injured than the top-masts, and the top-masts more frequently than the top-gallant masts: hence, although the Rodney and some other ships have been struck *above* the top-mast, it is obvious that lightning more frequently strikes the rigging below the top-mast-head than above it; and by taking into account the damage done by the mere falling of the top-gallant mast, as a consequence of the masts below it being struck and injured, it is highly probable that the cases in which lightning strikes

the spindle at, or above, the top-gallant mast head, bear a very small proportion to the cases in which lightning strikes the sides of the masts and yard-arms. From this inference, it will be obvious that an *oblique* flash of lightning striking the body of the rigging, could not arrive at a conductor, let into the wood on the *after-side* of a mast, without damaging the mast itself; unless it came from a cloud astern the vessel. Were the lightning to strike any of the yard-arms in order to arrive at a conductor, that yard-arm would receive as much damage as if no conductor had been there: and it is even possible that the conductor would be the means of increasing the damage, by causing the lightning to run along the whole length of the yard-arm to the mast; and the mast itself might then be traversed by the lightning and shattered between the yard and the conductor. The sails, ropes, spars, and every article which the lightning traversed on its way to a mast, would receive precisely the same extent of damage as if no conductor were attached to it. And men placed in, or near, the track of the lightning would be as sure to receive a death-blow as under any other circumstance in which lightning entered the rigging. Moreover, as these *central* conductors would offer increased facilities for lightning to strike the masts, all the evils usually attending oblique discharges through the rigging to them, would necessarily be increased in like proportion.

206. To whatever species of force we may be disposed to allude, the bursting of the massy iron hoops on the main-mast of the Rodney (183), we are compelled to infer that a similar force operating on a mast with its conductor, would be productive of similar effects, to a certain extent. If we are to suppose that the hoops were destroyed by the force of a lateral explosion of the first kind (193), then a similar lateral explosion between the mast and its conductor, would shatter both of them about the place where the lightning struck the mast: and it is not overstepping the boundaries of electrical evidence to infer, that the *closeness* of the conductor to the mast, would be one circumstance at least, conducive to the destruction of the latter. The iron hoops on the main-mast of the Rodney were close jambed to the wood, and situated under the best circumstances to be split open from a sudden expansive force within. Upon the same principle a conducting strip of copper, close jambed to the wood within a groove in the mast, might probably, not only be burst asunder, but peeled from the wood for many feet upwards and downwards, from the point where the lightning struck the mast. No circumstance could better substantiate the veracity of this inference than

the following, which is so clearly described in the accounts from the Rodney. "The copper binder round the truck was burst asunder, a small piece broken out of the truck, and one of the metal sheaves for signal halyards slightly fused." In this case the fused sheave was *within* the boundaries of the "copper binder," and as it is highly probable that the lightning was divided between the two masses of metal, in this part of its course, the force of the lateral explosion occasioned by that portion only, which passed through the sheave, was sufficient to burst asunder the exterior copper binder. If it should be discovered that the copper binder of the truck was sufficiently heated to render it brittle, such circumstance would certainly facilitate the rupture of the metal; but by no means invalidate the inference I have drawn respecting the effects of the lateral force: whilst at the same time it would be an additional proof of the astonishing calorific effects which lightning is capable of producing on the best metallic conductors. The manner in which the fragments of the iron hoops were magnetized, if we consider the lightning to have *descended*, would prove that it traversed the mast *within* the hoops, and its lateral force the cause of their being torn asunder (183, 184).

207. The electro-magnetic effects occasioned by a flash of lightning traversing conductors which pass through the hull of the vessel would be exceedingly injurious to the chronometers on board; by magnetizing every piece of ferruginous matter which enters into their construction; and thus deranging their performance to an irremediable extent, whilst those pieces remained in the instruments. Not only would the *principal* conductor in the mast, be productive of these effects, but every strap, knee, and other metallic appendage to that conductor, would communicate *permanent* magnetism to all those morsels of steel which form so essential a part to those valuable horological instruments. The chronometers on board the Beagle, surveying ship, to whose safety Mr. Harris ascribes the instrumentality of his conductors;* are,

* "The report from the Beagle shows that the conductors had performed their office and defended the ship; this, it must be allowed, was of the utmost, perhaps vital, importance to the survey on which she was employed; since, if the vessel *had been struck* by lightning, it is more than probable that the many valuable chronometers, compasses, and philosophical instruments of various kinds, necessary to the survey, would have been seriously damaged: the cabin set apart for the charts not being far from the main mast." *Harris's State of the Question &c. Section E. p. 7.*

The above paragraph would imply that the Beagle was not struck by lightning: but as Mr. Harris has elsewhere given a different

by the circumstance of their not being injured, almost certain evidence that the ship was *not* struck by lightning at those times when the "hissing sound was heard on the masts" (185). Had a discharge of lightning traversed the main-mast conductor, every chronometer in the cabin would have suffered from its electro-magnetic influence; and no plan or invention whatever, excepting such as would distribute the electric influence on every side alike, could possibly prevent chronometers from being subject to these magnetic effects.

208. Mr. Harris is of opinion that the light which appeared to run down the masts of the *Dryad* (185, 186), was a decisive evidence of the ship being "struck by lightning in the common way, but without the ordinary ill consequences:" and says, "the electrical agency seems to have fallen so powerfully on the masts, that it produced about them a luminous atmosphere; a phenomenon not uncommon when large quantities of electricity traverse conducting bodies, in a heated or rare state of the air, such as that usually found on the coast of Africa."*

209. It is true that rarefied air, whether hot or cold, will facilitate the production of a luminous atmosphere around a conductor carrying an electric discharge; but this is not the case with *unattenuated* air, though of a high temperature: but on the contrary, heat offers a decided resistance to the display of the phenomenon and confines it within narrower limits than it would otherwise expand to and occupy. Unless, therefore, it can be shown that the barometric column was exceedingly low indeed at the time of the occurrence, Mr. Harris's explanation is by no means applicable to the case. Neither would a luminous atmosphere, such as Mr. Harris calculates on, have any resemblance to the phenomenon described by Captain Turner: for instead of its appearing like lightning running down the mast, the phenomenon would have been a momentary glowing column of electric light. We cannot produce anything like a *running light* when the conductors are sufficiently good and capacious to conceal the motion of the fluid, though such a phenomenon may easily be produced by the employment of inferior conductors.

210. I have seen a globe of electrical light traverse the surface of a wet silken cord, upwards of three feet long. One

opinion (186), it is possible that this passage was intended to state that those instruments would have been damaged "if the vessel had been struck by lightning" and *not furnished with his conductors* at the time.

* "State of the Question relating to the protection of the British Navy with Lightning Conductors."

end of this cord was tied to the lower end of a kite string, in which a strand of copper wire was laid, and the other end was tied to a young tree. Some non-commissioned officers of the Royal Artillery, who were looking on, thought that the electrical globe was about the size of a musket ball. The lightning was very heavy at the time; but neither the kite, which was only about fifty yards high, nor the string, were ever struck by it. The hissing noise in the string could be heard at the distance of a hundred yards.

211. On one occasion, at Maidstone, a continuous stream of dense sparks traversed the same silken cord when quite dry, for several successive minutes, whilst a cloud passed over the kite. The string was cut, or burnt, at about three hundred yards from the ground, and the kite lost: but no lightning was seen, nor thunder heard, at the time. More than fifty persons witnessed this fact. We heard thunder at a distance about some ten minutes after the cloud had passed over the field where we were experimenting.

212. At Kirby Lonsdale, in the spring of 1834, a man named William Croft, was severely struck by a discharge from my kite string, when no lightning was present. The electric fluid discharged itself over a four-foot long stick, which Croft held in his hand, pointing the farthest end to the string. A hail shower was falling at the time. Several hail showers fell the same day from well defined insulated clouds, leaving the sky quite clear after the transit of each. Long dense sparks could always be had at the kite string on the approach of a cloud, and during its transit over the kite: but only very feeble electrical indications could be obtained when the sky was clear for some distance about the kite.

213. Under similar circumstances, at Addiscombe, I have had rapid *spontaneous* discharges from a two-gallon jar, whose inside was connected with the kite string.

214. I once received a severe blow from a kite string; when no visible cloud was within a mile of the kite, although the string, containing a wire its whole length, was uninsulated (tied to a tree) at the time, and at the distance of three feet from the place where I stood. The discharge took place over a dry silken ribband with which I was attempting to lower the kite. I had received several shocks before this heavy blow which was so severe on my chest, thigh, and shin bones, that I was deterred from taking down the kite, until a cloud, which I observed to windward, had passed over to a great distance on the leeward side. It was a thin ragged cloud, and did not discharge any lightning. This fact occurred in the Royal Artillery Barrack field, at Woolwich,

215. I have been particular in describing these facts, because they show that those phenomena which Mr. Harris considers as sure indications of his conductors being struck by lightning on board the *Beagle*, and *Dryad*, are more likely to have been the mere consequences of *electrical waves* produced by clouds, or neighbouring lightning. Lightning invariably causes electrical waves, but is not absolutely necessary to their production, nor to the production of the phenomena in question: and as Mr. Harris has not taken any such facts into consideration, they will appear more important in this discussion: because if he has not been aware of them before, they may now possibly have a tendency to give him very different views to those which he has hitherto taken respecting the *cause* of the phenomena which were observed on board the above named two vessels.

216. It is highly honourable to Mr. Harris, however, that he has, in the most liberal and open manner, given a fair and ample invitation to investigate "the state of the question relating to the protection of the British Navy from lightning" by that system of conductors which he has proposed.* At the present crisis, a more important scientific topic could not well be imagined: nor can there be one which has a greater claim upon the serious consideration of every experienced electrician. I have been induced to respond to Mr. Harris's solicitation in the hope that, from my humble example, others, more competent than myself, and with more ample means at their disposal, will be induced to take up the subject. I hope, however, that I have succeeded in pointing out some important truths which, previously, were either not known, or unaccountably neglected: and that I have been enabled to place the true character of Mr. Harris's plan of conductors, and the experiments intended for the illustration of their superiority over others, in a much clearer point of view than any in which they have hitherto appeared,

217. Having no motive, beyond that of the public good, for introducing his conductors to the Navy in preference to other systems, Mr. Harris will feel a gratification at his solicitations to enquiry being thus promptly and diligently attended to: and that this great question has been fairly and impartially discussed; that mere speculation has been stu-

* "It is therefore further submitted, that Mr. Harris may justly claim to have these circumstances fairly considered. In seeking for an enquiry, he aims at nothing which may not come openly and fairly before the country, without any kind of reservation whatever." *State of the question, &c.*"

diously avoided ; and that no inferences have been drawn but such as either rest on incontrovertible data, or have ample probability in their favour.

Description of a new system of Lightning Conductors for Shipping.

218. By the preceding investigation we are enabled to understand that narrow strips of copper let into the masts afford but a very partial protection to a great part of the rigging, and are liable to serious injury from oblique flashes of lightning striking the masts (205, 206); and that, in order to prevent all hazard of leading destruction into the very vitals of the ship, it is more prudent to conduct the lightning entirely away from the interior of the hull than directly through it: and as clear from the plane of the masts and as exterior to the principal parts of the rigging as circumstances will allow. Hence, therefore, had the frequent hauling up of chain or other flexible conductors to the masts' heads not been a serious inconvenience, those metallic chains, or ropes, if sufficiently numerous, would probably have been as secure a protection to vessels as any that could easily be devised: for instead of permitting an oblique discharge of lightning to traverse a portion of the rigging with the probability of splintering a yard arm, or a mast before it arrived at them, they would, by their *exterior* position, envelop within their boundaries, not only the masts but the principal rigging of the vessel, and shroud them from the effects of the discharge: and no plan of protection to the masts, from a *perpendicular* discharge of lightning, could be more perfect than a system of continuous conductors hanging from the masts' heads, and passing over the sides of the vessel into the sea. As, however, the hauling up of such systems of conductors, and the liability of their being an obstruction to the working of the sails, &c., are strongly urged as objections to their general use: and that *fixed* conductors are absolutely desirable, not only to prevent occupying the time of the men when wanted for other duties of the ship, but also to be in constant readiness to parry the effects of sudden or unexpected electrical storms, we should endeavour to employ all such means as are available to accomplish this great object: and as simplicity and security ought to be the leading features of every plan, and economy always kept in view, I am not aware of any plan so likely to meet the demands of the circumstances to so great an extent as the simple system of conductors which I now propose to describe.

219. Nothing can appear more simple than to attach fixed conductors to the lower rigging, because the lower masts are always standing under every circumstance of weather. I therefore propose to protect the lower masts and rigging by cylindrical rods of copper; four to each mast, and situated exterior to the shrouds, having one *before* each fore-shroud, and one *aft* each after-shroud. The upper extremities of these conductors to be attached to the fore, main, and mizen tops, as distant from the masts as circumstances will allow: and in any manner most secure and convenient. The lower ends of these copper rods to be fixed to the chains on the outside of the fore and aft shroud of each mast: and continued, by broad and stout straps of copper, to the copper sheathing of the vessel. By these means both the *starboard* and *larboard* shrouds of every mast would each be flanked by two conductors, which would be always at their posts ready to receive any flash of lightning tending towards the masts, from whichever side of the ship it might approach. As these conductors might be made to extend to some distance *fore* and *aft* of their respective shrouds, without obstructing the working of the lower sails, they would form an alinement on both sides of the vessel so as to protect each other's masts as well as their own. To prevent lightning from entering the lower rigging from ahead, I propose a conductor on each side of the fore-stay, their upper ends to be united with the conductors of the foremast, and their lower ends with the sea in the most convenient way. The whole of these conductors would be as permanent, and as unobstructive to the working of the vessel, as any part of the standing rigging; and, with the exception of the after part, would so completely enshield the lower rigging, as to parry any flash of lightning tending towards it. The lower yard arms would however still be unprotected, though not so liable to receive injury from without, as by Mr. Harris's system; because, in every position, they would be near to some of the conductors, which would relieve them of any discharge they might receive before it arrived at the mast. Yard arms would be very easily protected by flexible conductors uniting them with those which are permanently fixed. By such a simple system of conductors the whole of the lower rigging would be as completely protected from lightning as any system of *fixed* conductors are ever likely to protect it: and certainly without the slightest chance of injury to the lower masts or to the hull of the vessel. For though there were no conductors in the upper rigging, and that lightning were to strike an upper mast, it would do no injury below the lower mast head; as it would there enter the system of con-

ductors, which would convey it over the edges of the vessel to the sea.

220. The topmasts and rigging are easily protected by a system of conductors similar to those already described for the lower rigging, and may remain permanently fixed as long as the topmasts are standing. They may consist either of inflexible rods or of flexible metallic ropes of sufficient dimensions, four to each topmast. The lower extremities of these conductors are to be united with the upper extremities of the lower ones; and their upper extremities to the cross-trees. The upper and lower systems of conductors may be so continuously united, by the former sliding on the latter, as to present no inconvenience whatever, even under circumstances necessary to strike or to remove the topmast. By these systems of conductors every part of the ship, below the topmasts' heads, would be well guarded; and without the slightest risk of danger from the presence of the conductor; and as lightning but seldom strikes the rigging higher than the cross-trees, it would not be of much consequence to carry the conductors higher up. Oblique flashes of lightning which might otherwise strike the top-gallant mast, would be directed to the topmast head when furnished with these conductors.

221. As, however, every chance of danger to the men, and every species of damage to the vessel ought strictly to be avoided as far as practicability will admit, it still appears desirable to furnish the top-gallant rigging with conductors: and perhaps those which would give the least trouble to the men, would be strips of copper let into groves in the masts, according to the plan proposed by Mr. Harris; but instead of one strip only to each mast, I should propose *three* in each, at equal distances from each other; which, by having an exposure of metal on every side, would be a greater security to the mast than by its having one strip only. These strips of copper need only be narrow, and of moderate thickness; as they would be made to act in concert by uniting them at, or near to, the truck, with a copper band. A similar band would unite them at the lower end of the mast. Precautions similar to those so admirably contrived by Mr. Harris, for maintaining metallic contact between the top-gallant conductors and the system next below, must, necessarily, be attended to. The strips of copper should be continued to the vane spindle.

222. Four cylindrical copper rods, or four flexible metallic ropes, stretched from the cross-trees to the truck, parallel to the top-gallant shrouds, would afford a much better protection to the top-gallant rigging, than any conductors let into the

mast. Flexible conductors would be as easily set up, or taken down, as the shrouds themselves.

223. The bowsprit would be very well protected by the conductors accompanying the fore stay (219), they being continued on each side near its lower surface, to the sheathing at the bows; and the jib-boom would be protected by three copper ropes, or chains, reaching from its extremity to the conductors of the bowsprit. The jib-boom, however, being so little liable to be struck by lightning, presents no very strong claims to attention.

224. Let fig. 8, Plate IV, represent a transverse section of a vessel through the plane of a mast. Then the lines *c c*, *c c*, will represent the conductors on the *after-side* of lower shrouds; and the inner lines *c' c'* and *c' c'* the conductors on the *fore-side* of the lower shrouds. The lines *t t* and *t t* will represent the conductors on the *after-side* of the topmast shrouds; and the lines *t' t'* and *t' t'* will represent the conductors on the *fore-side* of the topmast shrouds. The conductors of the top-gallant masts are too easily understood to need further illustration.

225. Having thus described a system of marine conductors which, to me at least, appear more efficient and less objectionable than any other hitherto offered to public notice; the next consideration would be the expense of equipment: which I will compare with the expense of equipment by Mr. Harris's plan. The following official document, which is copied from Mr. Harris's pamphlet, is highly valuable on this point; coming from the best authority it furnishes unexceptionable data.

226. *Documents relating to expense.*

The following are extracts from a report by a gentleman who was sent from his majesty's dockyard at Chatham, by order of the commissioners of the navy, to estimate and inspect the work; and who, being a naval engineer, and otherwise a person of great intelligence, was considered by them equal to the task.

"His Majesty's Dockyard, Chatham, July 9, 1834."

"Sir,—In obedience to instructions received from the principal officers of this yard, in conformity with your official communication of the 1st instant, I herewith enclose a statement (in a condensed form) of what would be the expense, in labour and materials, of applying Mr. Harris's lightning conductors to each class of ships, supposing them fitted at the most eligible time. I have likewise ventured to append a few cursory remarks, which seemed necessary in order to afford a criterion of what may probably be the *ultimate expense* to

the public, in the event of the plan being generally adopted in the service. With this view, I beg further to transmit schemes of prices for labour, for different artificers employed on the work, upon which it should be added the accompanying estimate has been made."

"I have the honour to be, &c., &c.,
W. M. RICE."

"To G. J. Smith, Esq., Secretary of H.M. Navy, &c."

Mr. Rice then proceeds to give a complete table of particulars in the fitting each part of a ship, according to the plan above mentioned, the whole of which it is not necessary to detail here. It will be sufficient to state the general results as they appear in the last columns of his table.

TABLE 5.

Class of Ships.	Total of Masts and Hull.					
	Labour, &c			Reconvertible Copper Materials.		
	£.	s.	d.	£.	s.	d.
120 guns	60	18	0	305	12	2
84 ..	56	18	0	292	19	11
74 ..	54	17	0	263	2	3
50 ..	50	16	0	235	17	3
46 ..	45	15	0	190	16	4
28 ..	38	14	0	123	13	3
18 ..	29	12	6	89	14	8
10 ..	24	10	0	77	16	9

83. Upon this report Mr Rice offers the following remarks:—

"This estimate is grounded on the supposition that Mr. Harris's plan be applied to ships at the most eligible time, viz: during the progress of building or repairing, when the essential or original fastenings of the hull may be made to subserve as conductors: much delay will be thus prevented, and many contingent expenses saved."

"As the prompt execution of work much depends upon the facilities afforded to the workmen, I beg, without further recapitulation, to refer particularly to the arrangements proposed in the letters and drawings which I had the honour, on a former occasion, to submit for the consideration of the commissioners of his majesty's navy; humbly conceiving that they will greatly assist the desirable object contemplated by Mr. Harris, inasmuch, as by adopting them, the practical process will be greatly simplified, and the general introduction of the plan much facilitated."

"It may be proper to make a few observations on the subject of expense. Referring to the accompanying condensed statement, the last column *but one* denotes the *money absolutely sunk* upon the first ship of each class fitted with lightning conductors, and the *last* column, the outlay upon *reconvertible materials*, which must be

viewed as other articles of a ship's furniture for the protection of lives and property, such as *life-buoys*, &c. &c., with due allowance for wear and tear. In taking, therefore, a prospective view of the expense, very considerable abatement will be made upon the first cost by return of copper into store, which it is presumed will have suffered very little deterioration after many years' service; moreover, the straps over the heads and heels of masts, nuts, and screws, vanes, and vane-spindles, and the branch conductors under the beams, when removed from any ship, may all be placed in store, to be again replaced in other ships of the same class; the plates taken out of masts may probably be made serviceable by redressing, or, at most, will only suffer a loss at the rate of one penny per pound for remanufacturing: the plates removed from ships' hulls may certainly serve for many ships successively. Hence, should the plan be generally adopted, the amount of labour will be much reduced by the appropriation of serviceable articles to several ships, so that the loss alone upon the material *must not be charged* again upon a second or third ship."

"The absolute expense to the public is really the aggregate of the amount of labour, &c., as money sunk, with interest, together with the interest upon the first cost for copper for a given time. It would be difficult to assign any scale of per centage for the protection of ships by lightning conductors; but, in order to form some notion, within certain limitations, let the value of a first-rate, when fully equipped, be assumed in round numbers as worth £110,000, the value of property simply considered, neglecting the consideration of lives, and the dangers and disasters arising from a disabled ship, and taking the average of five years' run only upon the principal and interest, the annual premium would be less than £30., making the per centage under *sixpence halfpenny*."

"If we extend the term of years, the aliquot part of the money sunk will of course be less, and therefore the per centage will be reduced in like proportion; but, if this view of the expense be carried forward to a second, or a third ship, the *money sunk* will be very considerably less, on account of the diminution of labour, &c., whilst the term of years is still further increased; and *therefore the premium or per centage, would be ultimately trifling*."

"(Signed) W. M. RICE."

"Chatham Yard, July 9th 1831."

227. Mr. Harris has given the following dimensions of his conductors for "one mast of a frigate of fifty guns."*

	Length.	Mean Width.	Cubic Inches.
On the Royal Pole	18ft. 3in.	2in.	82
On the Top-gallant Mast. 17	0	2·5	95
On the Topmast	50	0	4
On the Lower Mast	93	0	6
			1255

Total copper. 1882

* Nautical Magazine for November 1837.

228. By taking the specific gravity of copper at 9000; the weight of 1882 cubic inches will be 613lb nearly; which, at one shilling per pound (and it may be had at a lower price), would cost £30. 13s. This average for each mast, would give £92. nearly: the cost for copper on the the three masts of a fifty-gun frigate.

By referring to Mr. Rice's estimate for a similar fifty-gun ship, we find that the copper alone, costs nearly £236. Then $236 - 92 = £144$, is the cost of the appendages, mostly below deck, to Mr. Harris's conductors.

There is still, however, another consideration in these estimates. Mr. Harris's conductors proceed to the bottoms of the masts, and are the broadest below deck; therefore the quantity of copper *above deck*, in each mast, is considerably less than 1882 cubic inches: and its cost proportionably less than £30. 13s.

229. Now the quantity of copper on the lower mast is about two-thirds of the whole (227); and by allowing about one-third of that on the lower masts to be below deck, we have $(2 \times 92) \div 9 = £20$. 9s. for the cost of the copper in the masts below deck. Rejecting the nine shillings, we have $92 - 20 = £72$. for the cost of that portion of the copper conductors which are above deck.

230. Now this seventy-two pounds-worth of copper (229) is, of course, supposed by Mr. Harris, to conduct lightning safely to the deck. Then, as my system of conductors, on each mast, would act in concert as decidedly as if in one piece of metal; they, with the *same quantity* of copper, would stand the same chance of being efficient to the *chains* as Mr. Harris's to the deck: and consequently, the cost of the metal for my conductors, would be £72. with the additional cost of about £15. for the fore-stay conductors, and fastenings to the whole.

231. The quantity of copper above deck in Mr. Harris's system, does not appear to me to be sufficient, requiring at least one-half more. Hence the cost of the copper for the equipment of a 50-gun frigate, with my system of conductors, would be £130., calculating it at the rate of one shilling per pound, which is, perhaps, about one sixth too much; and consequently, the material for Mr. Harris's system, would cost more than double the sum required for mine.

232. The next thing to be considered is the expense of workmanship; which, for my system, could not possibly exceed £20.: which, being deducted from Mr. Rice's estimate of £50. 16s., would leave a saving of £30. 16s. But Mr. Rice's estimate is "grounded on the supposition that Mr.

Harris's plan be applied to ships at the most eligible time, viz., during the progress of building or repairing &c." (226); and, consequently, applicable under those circumstances only. Under all other circumstances, the expense would be enormous: the ship having to be completely dismantled above deck, and the lower masts taken out and removed from her. It appears therefore, from the whole, that under the best circumstances in which Mr. Harris's conductors could be applied to a 50-gun frigate the expense would be £286. 13s. To equip a similar vessel with the system which I have proposed would never exceed £150., reckoning the copper at one shilling per pound. At tenpence per pound the expense would be £128. Taking the mean of these prices, the expense of the two plans would stand as below.

Harris's system	£286.
Sturgeon's ditto	£139.

Difference .. £147.

For a 120-gun ship the difference
would be nearly £200.

233. The *time* necessary to equip any ship with Mr. Harris's system of conductors, would be ten times that necessary to equip a similar ship with the conductors which I have proposed. Moreover, the latter system could be fitted to any ship, on any station or on any service, either at sea or in harbour. I do not, however, advocate the cause of economy as a primary object; as the only gratification which I can experience must emanate from a consciousness of having made a proper application of the principles of science to the protection and welfare of my fellow men.

*Westmoreland Cottage, Pomeroy St.,
Old Kent Road, London.*

Sept. 8th, 1839.

P.S. The readers of the *Annals* will perceive that this is not the precise subject intended to form the fourth memoir. The subjects are similar, and this being the more important one I have given it the precedence. W. S.

Westmorland Cottage, Pomeroy Street,
Old Kent Road, London, Sept. 12th, 1839.

My dear Sir,

You are well aware that the subject of electricity has for a long series of years, occupied a great deal of my attention: and *marine lightning conductors* have been amongst my most careful studies,

though I have not published any of my ideas respecting them till very lately. The now apparent intention of introducing your system of conductors into the navy has induced me, however, to arrange the results of my investigations on this topic in a somewhat regular order, and as I was aware of your being at the Birmingham Meeting of the British Association, I was desirous of bringing my views to light under circumstances which would give you the best possible opportunity of correcting any errors which, by possibility, I might have happened to have fallen into. My paper would not reach Birmingham till Thursday evening, the 29th ult. but as you were on the committee, I have no doubt of its being immediately placed in your hands, and in sufficient time to peruse it, before it would be read.

You will have perceived that the results of my investigation are not favourable to your plan: they point to several objections which you do not seem to have been aware of, which, as they are plainly described in the memoir, do not require to be particularized in this place. If I have fallen into any error respecting the probable effects of lightning through the instrumentality of your conductors, it is in consequence of the supposition of the mizen conductor passing through the powder magazine. Although I have great authority on that point: yet as you have ample means of avoiding so dangerous a situation, I would not urge that objection; though I cannot find any data to conduct my reasonings to any other conclusion than that, *danger is always to be apprehended from lightning being led into the body of the ship*, not only from the calorific effects of the *main discharge* alone, but from the effects of the lateral discharges, which your system of conductors does not provide against.

Truth, and the welfare of our fellow men, being the only motives which either you or I can have in this undertaking, I have been particularly desirous that the results of my enquiries should appear in the most prominent and simple form; and I think you will acknowledge with pleasure that the candid and open manner in which I have freely discussed every point, can lead no one into error respecting the inferences I have drawn.

The subject is of such paramount importance to every maritime country, and requires such general attention and rigorous investigation, that I mean to submit the substance of my memoir to the consideration of the principal scientific bodies in Europe and America, in order that the subject may be fully sifted and clearly and satisfactorily explained by the ablest electricians which the world can at this time produce. I shall publish it entire in the October number of the "*Annals of Electricity &c.*" and shall feel much pleasure in publishing any remarks you may wish to accompany it, either in favour of your own conductors, or any you wish to make on that system which I have proposed.

I am, my dear Sir,
Yours very truly,
W. STURGEON.

To W. Snow Harris, Esq.
&c. &c. &c.

XVII. *Memoir on the employment of the oxy-hydrogen gas in its applications to the purposes of Chemistry and the Arts, by means of the Silex safety Blowpipe, an instrument adapted to the perfectly secure combustion of explosive gaseous mixtures in any continuous quantity, and with jets of extraordinary dimensions. By W. H. WEEKES, ESQ.,* Surgeon, Lecturer on Philosophical and Operative Chemistry, &c. &c. &c.*

Fertile in expedient and powerful in agency as the combined efforts of genius have contributed to render modern chemistry, it may, perhaps, be fairly doubted, when we except the mighty voltaic arrangements which enabled our celebrated Davy to decompose the alkalies and elucidate the basis of the earths, if, in the appliances of this almost universal science, we can rank one superior, or even equal in its general operation, to that which uniformly obtains from the use of the oxyhydrogen blowpipe, when its operations are guaranteed by safety, and its management is skilfully directed. Our present purpose is not designed to trace the history and progress of this astonishing instrument, which, in the hands of our chemists of the nineteenth century, and with the several improvements it has gradually received, exhibits such small comparison with its ancient form of construction and degree of effect, as scarcely to be recognized as belonging to the same class of philosophical apparatus. The daring essays, however, which have paved the way to this most important change, have too frequently been marked by the occurrence of alarming and serious accidents to the enterprising experimentalists, while engaged in their researches with those tremendous agents, the "mixed gases," as they have usually been denominated. Foremost in this little phalanx of bold and ingenious master spirits appears the honoured name of Dr. E. D. Clarke, whose thirst for chemical and mineralogical knowledge led him, while thus engaged, repeatedly to the detriment of his health and the imminent hazard of his life. Many persons as well as myself, I doubt not, will recollect an engraved representation of the Doctor's blowpipe, placed on one side of a thick deal wall or screen, with the jet pipe (the orifice of which seldom exceeded one fortieth of an inch in diameter) passing through the said wall into an adjoining apartment, where the operator directed its action in comparative security. Prudent and even necessary as this precaution must have been originally, future chemists will probably view the engraving to which I have

* Communicated by the Author.

alluded with a smile combined with feelings of wonder and congratulation at the march of scientific acquirement. The extremely hazardous character of Dr. Clarke's blowpipe, yet notwithstanding his brilliant experiments and discoveries therewith, speedily turned the attention of eminent scientific chemists to its adoption and improvement. It would be not only tedious, but, at this day, equally unnecessary to recapitulate all the ingenious expedients which have in turn been rendered available, with various degrees of success, for the purpose of guarding against and subduing the explosive properties of oxyhydrogen gas in its applications as an agent of chemical research. Our brief consideration of the principal methods hitherto in use, may be reduced mainly under two heads. First, the employment of separate reservoirs to contain the respective gases (subject to condensation or otherwise) which are made to unite in, as near as possible, the component proportion for forming water, immediately previous to their exit from the extremity of the jet pipe and their consequent ignition. This method has doubtless the merit of perfect security, but, though I have devoted no small share of attention to experiments of this description during the last twenty years, I have never been able to obtain upon this principle so great or so uniform degree of heat, as when I have employed the gases in a state of previous admixture; nor does it appear practicable thus to unite them, however ingenious the means, with sufficient accuracy to ensure the fullest effect to which they are competent when burnt from a single gas holder. The second class of securities to which I have alluded embraces the object of combining the gases in due proportion, previously to their being conveyed into the blowpipe, and then preventing a retrocession of the oxyhydrogen flame, by the interposition of a safety medium. Now it is a well known fact among chemists that this gas will not inflame below a certain degree of temperature; the heat yielded by a piece of common touchwood in a state of glowing ignition, for instance, will not set fire to a stream of this gas, unless the wood can first be excited into a flame; therefore whatever substance will allow of the free passage of the gas through its interstices, yet at the same time invariably reduce the temperature of the burning jet below the ordinary point at which its combustion obtains, will necessarily preclude its return in an ignited state to the gas reservoir, and thus constitute a security against all explosions. To accomplish this desideratum, series of wire gauze, tubes charged with iron or steel filings, and, above all, a congeries of small wires wedged longitudinally into a brass tube, have repeatedly been adopted. I may permit myself to remark,

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and it will certainly be without a shadow of invidious feeling, that several years prior to these several modes becoming public, they had a fair trial in the course of my own experiments, but in my hands they were not invariably successful in preventing explosions. With the congeries of longitudinal wires, for a time, all went on well; but if by any chance moisture found its way among them, or they became oxidated, a consequence which assuredly followed by frequent use, then the certain finale was a "blow up," and the dispersion of my apparatus in fragments throughout the laboratory.

Now, as all projectors and inventors doubtless feel a much greater degree of confidence in their own schemes than can be immediately appreciated by other people, I shall forbear to expatiate on what I conceive to be the merits of an apparatus about to be submitted to intelligent and unbiassed men of science; contenting myself with simply remarking that I have successfully employed it during many years past and in some thousands of instances. Its security I have found *absolute*, and its powers such as I would rather leave to be decided by experience than resting on my own assertion.

Description of the Silex-safety Blowpipe.

Fig. 1, Plate V, represents a central perpendicular section of the instrument and its immediate appendages, the precise nature of which, I think, may be rendered perfectly intelligible without the aid of letters of reference, provided the reader will place the figure before him while he peruses the subjoined particulars. The body of the apparatus consists of a cylindrical vessel, eleven inches in height and four and a half in diameter, formed from sheet copper about one-sixteenth of an inch in thickness, and is firmly supported by a circular foot or base of the same material, projecting to the extent of one and a quarter inch from the perpendicular line of the cylindrical vessel, the bottom whereof terminates the horizontal plane from whence the projecting base commences. The top of the instrument, or superior circular disc of the cylinder supporting the tubular appendages to be next described, is not permanently soldered into its place, but is provided with a rim which descends within side of the cylinder to the depth of three-eighths of an inch; and, by accurate grinding of the two surfaces in contact, is made to fit air-tight, or may be easily so rendered by the occasional use of any of the cements which become liquid at a low temperature. This arrangement is found advantageous, as, by the application of a spirit lamp flame for the space of a few seconds, the top

of the instrument may be removed and replaced with facility, and thus give free access to the interior works of the cylinder, though such approach can rarely be required. Consecutively with each other in a line drawn across the centre of the copper disc forming the top of the instrument, and with their lower rims permanently soldered therein, arise the three brass tubes seen in the sectional figure, surrounded concentrically at their respective bases by a second piece of tube which has the effect of rendering them severally secure and firm in their required positions. We will explain the object of each in succession, beginning with the one most conspicuous on the right hand. This tube is two and three quarter inches in height, and one inch and a quarter in diameter, furnished at the top with a milled-edge screw cap and a collar of leather for the purpose of closing it air-tight. From the side of this upright tube projects a second, three-fourths of an inch in length, and at right angles with the first, containing the socket which receives the screw of the accompanying stop-cock; the opposite end of the latter being in like manner attached to the base or commencement of the annexed safety-tube, three inches in length, and seven-eighths of an inch in diameter, from the right hand end whereof proceeds, to the distance of five-eighths of an inch, a stout brass projection properly perforated and communicating with the main tube, and into the cavity of this connecting piece are accurately ground the several jet pipes, straight and curved; a method whereby they are much more easily and expeditiously shifted for use, during the various operations to be carried on, than they can possibly be when attached in the ordinary way by means of screws. The extreme ends of the jet pipes (of brass and neatly turned) are made to terminate in a globular enlargement, as seen in the figure, whereby they are preserved from overheating and destruction when the gas flame is long continued upon refractory bodies placed upon charcoal or other supports. We come now to describe the central tube of the three, seen in the section before referred to: it is one and three quarter inch in height, and one inch and an eighth in diameter, covered by a brass plate at top, through the centre of which is drilled a circular aperture, three-eighths of an inch diameter at its superior edge, and decreasing downwards, so as to receive an inverted cone of solid pewter, carefully turned to fit perfectly air-tight when the requisite degree of pressure is applied. The mode of obtaining this pressure is simple and effective: from the inverted base of the pewter cone, and secured thereto by means of solder, ascends a small brass wire until it reaches an inch or little more above.

the horizontal cross piece of the right angled vertical stays, which are attached, by means of a small screw, on each side of the main tube, a little below the edge of its circular top. Over this wire, to the height of three quarters of an inch, and resting loosely upon it as well as the inverted cone, is placed a helix consisting in five or six spiral turns of very elastic steel wire, about one-sixteenth of an inch in thickness. Again, upon the upper extremity of the helix rests, also loosely, a circular disc of brass, three-eighths of an inch in diameter, and one-eighth in thickness, its upper surface being a perfect plane, while underneath it is shaped somewhat concave with a depending rim, to retain in its proper position the upper termination of the helix before mentioned. Upon the upper surface of this small brass disc is resting the end of a screw which is hollowed longitudinally, and passes freely over the perpendicular wire arising from the inverted conical valve. This hollow screw has a delicately cut thread on its external surface, and, when turned by its milled-edge nut on the top, revolves about the perpendicular wire, and works into a corresponding socket in the horizontal cross piece of the vertical stays; and thus, by means of the spring helix, regulates with perfect facility any degree of pressure which it may be desirable to put on the conical valve. The third or small tube on the left hand may be one and a half inch in height, and about five-eighths of an inch internal diameter; it is simply closed by a projecting milled-edge screw cap and a collar of leather, for the purpose of ensuring it air-tight, when the apparatus is used. From the left-hand side of the copper cylinder, and at the distance of one and a half inch from the top thereof, proceeds a substantially made stop-cock, terminating outwardly with a screw in the usual way, or, at pleasure, with a hemispherical cap, having a smooth cylindrical cavity in its centre, for the reception of a corresponding tube, ground to fit air-tight. This stop-cock communicates, by means of the usual socket, with a small circular copper box, about one inch and a quarter in diameter, well secured by solder on the inside of the cylinder. From the back part of this box, and communicating with its cavity, descends, diagonally across the middle of the cylinder, a copper tube, three-eighths of an inch in bore, the lower end whereof terminates in a plane parallel with the base of the instrument; which plane is perforated with four or five holes the size of a large knitting needle, and reaches to within half an inch of the bottom of the cylinder; the tube being securely held in its diagonal position by an effective soldering to the back of the copper box above mentioned. To the distance of three-

fourths of an inch from underneath the centre of the bottom of the copper cylinder proceeds a brass tube of about five-eighths of an inch bore, as seen in the sectional figure, but necessarily concealed in perspective by the projecting base of the instrument. This tube in ordinary is securely closed with a good cock, and serves, upon the removal of the latter, as an outlet for the water charge employed when the apparatus is brought into operation in connexion with a reservoir of the mixed gases.

The instrument as hitherto described is, perhaps, as portable as need be desired, being wholly of metal, and weighing only fifty-three ounces, including stop-cocks, safety-tube, &c., charged for use, together with half a dozen jet pipes perforated in progressive degrees from one-twenty-fifth to one-tenth and one-eighth of an inch in bore, to suit the various purposes of the operator.

Preparation of Blowpipe and adaptation of Apparatus generally for use. (See fig. 1.)

Having closed the lower orifice of the copper vessel with a good elastic cork, the cap at the top of the left-hand tube is to be unscrewed, and, by means of a small funnel, the cylinder charged with water until its surface reaches to within three-fourths of an inch of the bottom of the tubes, leaving a circular area or space as denoted by the unshaded portion of the sectional figure. The funnel being removed and the left-hand tube again closed, the cap of the principal tube on the right is now to be removed, incidental to its being charged (not over tightly) with a piece of good soft and *slightly* moistened sponge of uniform texture, which being effected, the cap of this tube is also to be replaced and rendered airtight by means of its leather collar.

In order to procure the granulated silix for charging the safety tube already described, a quantity of what is commonly called "drift sand," a substance now found abundantly on all our Macadamized roads, should be subjected to repeated washings with clear water, until nothing but the silicious granules are found remaining. These, being first dried, are to be separated, the large and irregular from those appropriate to the purpose, by the use of a proper sieve which will allow of the passage of none but the finer and most uniform particles; with these the safety tube is to be *accurately filled*, so that no motion or change of place can occur among the granulations, when the tube is screwed to its connecting stop-cock, which being done, the jet pipe selected for the occasion may now be attached to the silix tube ready for use.

What I have next to communicate relates to the valve surmounting the central brass tube, and will, I anticipate, produce a smile from our scientific friends, when I inform them, as I now seriously do, that *it is a work of supererogation*; and, notwithstanding the pains I have taken in detail to render its construction familiar, I regard it as being utterly useless to the main intention of the instrument. The question will most naturally arise—Why then has it been adopted, and for what reason should it be retained? I answer that, though it can never be directly necessary to complete the security furnished by this form of blowpipe, it is, nevertheless, of considerable *indirect importance*. I am myself convinced to demonstration of the absolute security afforded by the interposition of the granulated silex, nor should I, for one moment, hesitate to discard even the water cylinder, and without any intervening medium whatever, attach my silex tube at once to a gas holder containing a thousand cubic feet of the explosive mixture, by the side whereof I should work with the composure which arises from implicit confidence, that dependence being not merely a theoretical consequence, but the result of more than twelve years' practical assurance necessarily begotten by experiments repeated almost ad infinitum. But I have no right to expect that my assertions alone, or the evidence growing out of my individual experience, can, in the absence of ocular proofs, produce conviction to the minds of others, especially in a pursuit fraught from its earliest origin throughout with a frequency of the most tremendous explosions and their usual alarming train of effects. Mr. Hemmings, several years since, in speaking to a scientific friend, of his apparatus for burning the mixed gases, said "his friends were afraid of him, and, although *he* felt quite safe in using his tube, even with a Pepy's gas holder, yet those near to him always made a simultaneous movement when he began to operate." Many a time and oft, not only in the lecture room but during the delightful hour of association with a knot of friends in the privacy of the laboratory, spite of theory, demonstrations of security, and the exhibition of cool confidence on my own part, have I witnessed this "simultaneous movement," when about to employ my silex tube for the deflagration of the combustible gases.* It is, therefore, not only

* Among the personal friends whom I have the pleasure to recognise as having devoted their talents to the advancement of this department of chemical science, I cannot but recall to my mind, with a feeling of extreme satisfaction, the name of Mr. T. O. N. Rutter, of Lyminster, Hants. This gentleman is the inventor of a very improved species of oxyhydrogen blowpipe, whereby the

essential to provide an absolute security but also absolutely essential to produce an ocular means of conviction that such security exists, guaranteed even to a threefold degree, ere we can hope to render the oxyhydrogen blowpipe extensively employed, and while we disarm the natural timidity of inexperience, to place it beyond the power of the inadvertent or careless operator to compromise himself and his friends in any more than a harmless pop-gun explosion. These, then, are my reasons for retaining the valve already described, and which may be adjusted with remarkable facility to any degree of pressure, simply by turning the milled-edge nut of the vertical screw previously explained in reference to fig. 1. We shall presently have occasion to recur to the subject of this valve, when treating of the operation of the instrument; but I will avail myself of the present opportunity to suggest to my scientific friends, that they may find this description of safety valve extremely simple in its action and effective in practice, as applied to philosophical apparatus on many occasions, wherein its *safety* properties will be more expressly operative and desirable than in the present instance of its application.

The reader is now referred to fig 2, Plate V. The arrangements hitherto prescribed having been carried into effect with

respective gases are condensed into separate copper reservoirs of large dimensions, and ignited at the instant of their union in a common orifice analogous to the principle some years since adopted by Professor Hare, of Philadelphia. One of these instruments constructed to order by Messrs. Jones of London, weighed, independent of the condensing syringe, 53 pounds; thus providing for long continued action, certainly, in numerous instances, a most important consideration, without even the shadow of danger from explosions. In reference to the point of my remarks in the text, I am sure my friend Rutter will excuse me, if I quote a few lines from one of his communications to me a few years since, as nothing can more forcibly illustrate the frequent danger, as well as the general feeling of apprehension necessarily connected in ordinary with gas-blowpipe experiments. I must premise that, as a mere memento of esteem, I had forwarded to Mr. Rutter one of my sillex safety tubes for his acceptance and trial. He says, "Although I have your assurance that your sillex tube is *safe*, I cannot conceal from you that I shall use it with 'fear and trembling.' An accident that happened to me in May, 1832, makes me very careful of explosive mixtures. I had a 'blow up' that left me bleeding out of about forty different places, spoiled an entire suit of good clothes, disfigured about fifty volumes of books, stripped the ceiling of some of its plastering down to the bare laths, and left other marks that will not be easily obliterated, to say nothing of a glass receiver, a porcelain ditto, and various other articles of apparatus which were totally destroyed."

the requisite degree of precision, the blowpipe is to be placed towards the left-hand end of a strong and rather heavily formed table of convenient height, which, from its weight and resistance of casual disturbance, will constitute a material advantage to the operator. The necessary connexion, between the blowpipe and the gas holder to be employed on the occasion, is now to be established by the intervention of an elastic gum tube from three to four feet in length, or of greater extent, at pleasure; one end of the tube being attached by an ordinary screw-socket to the extremity of the stopcock from the gas holder, and the other, by means of a *union joint*, to that of the blowpipe. In working I prefer to place the gas holder completely within reach of my left hand, as being thereby more under my immediate command and proportionately conducive to convenience. The various gas holders acting upon the hydrostatic principle—such, for instance, as the ingenious and (generally) valuable invention of Pepy's—I find are by no means well adapted to the purposes of operation with the oxyhydrogen blowpipe. Their use not only involves the necessity of an assistant, but it is utterly impossible to obtain by their means an equable and uniform degree of pressure; both of which objects are secured to a single operator by the use of the gas holder and gasometer shown in the figure; and, though the one I am in the habit of employing (capable of containing several cubic feet of the explosive mixture) differs in some few respects from those in general use among chemists, I am sure its operative principle is too obvious and generally known to require from me any especial description. The arrangement of apparatus herein suggested is, I believe, as compact and commodious as can well be desired, either for the purposes of the public lecturer, or those of private manipulation; the blowpipe, owing to its perfectly flexible medium of communication with the gas holder, being readily brought into any required position, or placed at the most desirable degree of elevation as respects both operator and observers.

Operative management of Apparatus.

The preliminary arrangements being completed, let the operator place himself alongside the table and right abreast of the blowpipe; he will then with his right hand have entire command over the ignited jet, his several supports, and the substances to be reduced or otherwise acted upon; while with his left hand he will secure the management of the gas holder and regulate his supplies therefrom. Let him commence his

operations by loading the inverted gas bell with the requisite degree of weight ; this done the stopcock next the safety tube is to be turned open, either wholly or in so much as may be deemed adequate to the especial occasion, subject to subsequent correction in working the instrument. The communication with the gas holder should now be established by the operator opening the other two stopcocks in succession with his left hand, and he will find there will immediately follow a continuous low gurgling sound, occasioned by the passage of the gas down the interior copper pipe, formerly described, and its re-ascent through the water of the cylinder. He may, without further delay, apply a taper or lighted match to the extremity or knob of the jet pipe, when a cone of flame (regulated by the degree of pressure on the inverted bell, the opening in the airway of the stopcocks, and the orifice of the jet employed) from two to ten or twelve inches in length, and of proportionate diameter, will be brought into instantaneous play upon the subject of his experiments.* So long as it is intended to continue the instrument in action, notwithstanding a temporary suspension of the gas flame may occasionally prove to be requisite, it will be unnecessary to interfere with the stopcock proceeding from the gas holder, and communicating directly with the elastic tube. The left hand stopcock of the blowpipe being closed—and it will be infinitely preferable to cut off and regulate the supply to the jet pipe principally by means of this stopcock—the gas flame will in the next instant become extinguished, and its disappearance be accompanied by a faint and harmless report only, proceeding from an explosion of the last portions of gas remaining in the tubular cavity between the knob of the jet pipe and the anterior extremity of the silex safety tube.

* These lengths of flame and even greater are safely attainable from the use of this instrument ; but it is a mistake to suppose that this circumstance is of any practical importance. The point of greatest heat is in fact to be found near the orifice of the jet pipe from which the gas issues. But though the length of the gas flame be of secondary consideration to the operator, whatever tends to increase its diameter is of the first-rate advantage, as it will enable him not only to bring much larger bodies within the sphere of its action, but he thereby obtains a very great augmentation to the degree of heat. In this particular respect the blowpipe now in question affords far greater facilities than any other form with which I have the pleasure to be acquainted, wherein a single gas holder is used, especially when the gas flame is directed downwards upon any substance rather than in a horizontal line.

Observations on the principle of safety.

In the sectional representation, fig. 1, it will be perceived, as we have already had occasion to mention, that the water line terminates at the distance of three-fourths of an inch from the top of the copper cylinder, the intervening space being occupied by atmospheric air, until that is displaced by the introduction of the mixed gases, a result which speedily follows when the apparatus is put into action. This provision is necessary in order to accommodate an increase in the volume of water which immediately obtains from the continuous ascent of gas bubbles as they emerge from the orifices of the diagonal tube, and which water would otherwise rise into the main upright tube charged with sponge; the latter substance effectually prevents any moisture whatever, occasioned by the breaking and splashing of the bubbles as they rise to the surface, from finding its way into the air passage of the stopcock, or insinuating itself, in consequence, among the granular silex of the safety tube. Now, *supposing*, for the sake of illustration, that the ignited gas jet could from any circumstance recede and burn backwards through the silex tube, the sponge medium (though not here designed for such purpose*) is amply capable of extinguishing it; or, allowing a second supposition, that the sponge should fail in arresting the course of the fiery renegade, the only mischief he could possibly effect would be the explosion of an insignificant quantity of gas, from three to four cubic inches in amount, by the force of which the conical valve in the central tube would be instantly raised, and the final consequence a harmless report. I presume it would be superfluous to enter on any argument in order to show that such explosion, even if it were possible in itself, could in any way extend to where the only source of real danger resides, viz. in the gas holder or reservoir connected with

* Many years since I placed at the disposal of the talented editor of the "Mechanics' Magazine," J. C. Robertson, Esq., an essay on the combustion of the mixed gases, with the description of an apparatus for their secure deflagration, in which the safety medium is constituted solely by sponge. I believe this essay appeared in Vol. VII. of the popular and valuable work to which I have referred. Years of subsequent experience have only served to strengthen the confidence I then expressed relative to my sponge safety tube, provided the rules I have prescribed for its management be *implicitly* followed; otherwise, through the sponge becoming dry and shrunken, &c. &c., explosions may and will happen, but the most incorrigibly careless operator might be defied to render inefficient the successive securities afforded by the silex safety blowpipe.

the blowpipe, as the flame would have in the first place to dive through a column of water nearly a foot in height, and then, further suppositions are obviously out of the question.

During a very considerable portion of my life, the oxyhydrogen blowpipe, in its various forms, has been to me a source of intense interest, both in reflection and practice; and few persons, it is probable, have ventured on a course of more hazardous experiments unattended by personal injuries or other serious contingencies. These fortunate and, in latter years, entire exemptions from accident, I certainly owe to the adoption of the silex medium, a substance possessing, in my estimation, the true character requisite for such a purpose, that of absolute *immutability*, whereby, if once brought into operation with suitable effect, the employment of the mixed gases is rendered an agency of really astonishing, if not unlimited, power in chemical researches and many purposes of the arts. With the identical instrument furnishing the descriptive portion of this memoir, the reduction of the several earths to their respective bases, and the consequent production of barium, calcium, &c., has not unfrequently been, as it were, the magical conversion wrought by a few seconds' application.

XVIII. *Description of an Electro-magnetic Engine.* By JAMES P. JOULE, Esq. *In a letter to the Editor.*

Broom Hill, near Manchester, 30th August, 1839.

Dear Sir,

I hasten to forward to you the description of my new electro-magnetic engine. I am particularly desirous of communicating any new arrangement, in order, if possible, to forestall the monopolizing designs of those who seem to regard this most interesting subject merely in the light of a pecuniary speculation.

Fig. 3, Plate V, represents a perspective view of the engine, in what may be termed its "quadratures." A, B, C, D, is a frame of wood made very firm, and strengthened by a board fixed at the back; the top E may, by means of thumb screws, be taken off at pleasure; the inside measure of the frame is $4\frac{1}{2}$ feet long, $1\frac{1}{2}$ feet high, and 1 foot broad. F, G, is the axle of cast steel, three quarters of an inch square, and turned to half an inch at the working parts. *a, a*, are holes by means of which the axle may with expedition be let into the brass steps *b, b*. H, H, are holders for the revolving electro-magnets *m, m*: there is another holder between these, which

is omitted in the figure, but which contributes very materially to the stability of the apparatus. n, n , are the stationary electro-magnets, firmly secured to the top and bottom by the brass clamps k, k . s, s , are silver springs which enter through the frame and press gently on the brass half discs of the commutator c . The commutator, figs. 3 and 4, consists of two half discs of brass, inlaid in hard wood: the silver springs being of the same size at the ends as the distances between the half discs; the electric current is instantaneously reversed. v, v , are copper wires, soldered to the brass half discs, and connected with the revolving electro-magnets. x, x , are wires let into the frame, and connected with the stationary electro-magnets. z, z , are wires to connect the coils of the two systems.

Fig. 5 will more clearly represent the plan of my holders; they are made of hard wood, and brass plates are screwed to the sides and faces; b, b , are brass screws, which work upon wedges, so that I can adjust the magnets at different distances from the axle; a is a screw which secures the holder to the axle.

The iron of the revolving electro-magnets is thirty-six inches long, three inches broad, and half an inch thick; that of the stationary electro-magnets is of the same breadth and thickness, but their length is such that their extremities when bent are $36\frac{3}{4}$ inches asunder; $3\text{-}8\text{ths}^*$ of an inch is therefore the working distance.

I shall provide myself with four of the straight electro-magnets, two of hard and soft iron, and two of hard and soft rectangular iron wire: the hard iron and wire magnets are completed and fixed on the engine. Fig. 6 represents the position and exact size of the wires, where a, b , are planed iron rods to secure them with certainty.

Each electro-magnet is furnished with a strip of thick calico next to its iron, and with three separate layers of well covered copper wire, $1\text{-}24\text{th}^\dagger$ of an inch in diameter, each 106 yards long. I always take care to ascertain that each coil is properly insulated by experiment, and the method which I employ in winding the wire ensures regularity.‡

My engine consists of only four electro-magnets, but it will at once be seen that the principle admits of the use of any superior even number; I do not however think that the mere

* $1\text{-}8\text{th}$ would be better.

† I have reason to think that this is too thin.

‡ A heavy weight is fixed to the wire, and wound up as the magnet is covered.

increase of number is attended by advantage, unless the friction of the axle is thereby diminished.

As the form of this engine enables me with ease to place the electro-magnets in different positions, and as the several coils are insulated, and I am thereby enabled to use the electric current in quantity and intensity arrangements, this engine is well adapted for experiment. It works beautifully, but as I have not as yet carried my experiments with it very far, I prefer to postpone for the present any account of its duty.

Vol. IV. page 132, line 23, read 62 instead of 62.

I remain, Dear Sir,

Yours most respectfully,

JAMES P. JOULE.

XIX. *Report on a Memoir, by M. MASSON, relative to the action exercised by chloride of zinc on alcohol.**

For some years past the question of the formation of ethers has been studied by chemists with so much care, and under so many different aspects, that it might seem difficult to find anything new in so simple a reaction as that which forms the subject of the present memoir; yet nothing was less expected than the results obtained by M. Masson which we are about to mention.

The author dissolved chloride of zinc in alcohol, and submitted the liquid to distillation, taking care to break the products, and keep exact account of their nature; but he found that in proportion as the liquid boils he at first lost alcohol, but when his point of ebullition, which was raised gradually, arrived at 130°, or rather 160°, it produced sulphuric ether.

Thus chloride of zinc acts on alcohol exactly like concentrated sulphuric acid; and what is very remarkable, these two bodies determine the production of sulphuric ether at precisely the same temperature.

Carrying the experiment further we discover an oil, which, in its character, exactly represents the oil known by the name of *sweet oil of wine*. It is formed at about 160°, i. e. nearly under the same circumstances as when we operate with sulphuric acid and alcohol.

We observe, further, that the ether disengaged is always accompanied by a certain quantity of water, and that it is

* From the Comptes Rendus, 24th December, 1838. Translated by Mr. J. H. Lang.

also the same with the sweet oil. These phenomena are also remarked in the reaction of sulphuric acid on alcohol. M. Masson is also assured that hydro-chloric ether is not produced, which fact he but little expected.

Hence M. Masson has perfectly proved that chloride of zinc acts like sulphuric acid. There now remains to consider a certain number of phenomena which the author has hitherto thought proper to neglect, and which take a great part in the reciprocal action of the sulphuric acid and alcohol. In fact, the analogy observed by M. Masson between chloride of zinc and sulphuric acid is so perfect, that it is difficult to believe that chloride of zinc does not furnish some product corresponding to sulpho-vinic acid. It is this that M. Masson has not sought to verify, and which we recommend to his attention.

Hitherto we have admitted that the author had obtained sweet oil perfectly similar to that obtained by the assistance of concentrated sulphuric acid. However, M. Masson has not confined himself to the establishment of this identity: he has considered the oil that he has obtained, and is satisfied, by attentive distillations, that it contains two very different products.

The first, the most volatile, is carburet of hydrogen, the most hydrogenated liquid known: it contains more hydrogen than olefant gas, and is represented by $C^s H^9$; it boils at about 30° or 40° .

The second, the less volatile, contains, on the contrary, less hydrogen than olefant gas; and is represented by $C^s H^7$, and boils only at about 300° .

These results joined to those by which M. Regnault has demonstrated the absorption of oxygen gas by sweet oil of light wine, will perfectly explain why some chemists have obtained from its analysis more carbon than is contained in olefant gas; and others, on the contrary, have hit on the composition of olefant gas itself.

These facts which appear to us well founded, would have led your Commissioners to regard M. Masson's work as of a nature to settle the discussions relative to the sweet oil of wine; but M. Marchand, a German chemist, who has been much engaged with sulpho-vinates, has recently published some analyses of the oil of strong wine, as well as some analyses of the oil of light wine, or some crystals which it furnishes. His results agree perfectly with those of Serullas, and, consequently, differ from those obtained by M. Masson under the inspection, and in the laboratory, of your reporter.

Considering that among the chemists who engaged in this subject, some have operated on the oil obtained by sulphuric

acid and alcohol; others on the oil of sulpho-vinates; and that M. Masson has procured his by alcohol and chloride of zinc, some might perhaps imagine that these several oils differ among themselves.

So much the more, since M. Masson has never been able to extract from his oil the crystals obtained by Hennell, Serullas, and Marchand, from theirs; and that, on the contrary, he has extracted a very volatile product unknown to the chemists who have preceded him.

But M. Marchand has undertaken to remove this last difference; for he distinguishes among the products of the distillation of sulpho-vinates, the existence of a very volatile product which he has not analyzed, but which appears to have the greatest relationship to that which M. Masson has discovered some time ago.

Hence, it is evident that the history of the sweet oil of wine is not yet terminated; but M. Masson has done a great deal in clearing from it the existence of a very volatile carburate of hydrogen $C^4 H^9$.

The reciprocal action of chloride of zinc and alcohol has been so well studied by the author of the memoir which engages our attention; it has been on his part the object of experiment so worthy the attention of chemists; he has so well proved that it gives rise to sulphuric and not hydrochloric ether, that we do not hesitate to request the insertion of his memoir in the "*Recueil des savants étrangers*."

The conclusions of this report have been adopted.

XX. *Employment of Gelatine for Food.**

M. Arago mentions that during his last visit to Metz, he received a letter from M. Darcet inviting him to visit the hospital of St. Nicholas, where they used gelatine for food: and he wished on his return to state to the Academy what he had observed. M. Arago subscribed at the desire of his professional friend, fearing, that during the examination of facts, he should be led astray by the influence of those prejudices which he had formerly entertained against the elementary regimen, the subject of so lively and prolonged a debate.

The hospital of St. Nicholas at Metz, contains more than five hundred persons, men, women, and children. The men and women are all very old. Each individual receives twice

* From the Comptes Rendus, 24th December, 1838. Translated by Mr. J. H. Lang.

a day, five days in the week, a soup containing *a quarter of a litre* (a little more than half a pint) *of broth*, which, for a thousand rations, is prepared with the gelatine produced from twenty-five kilogrammes (about $52\frac{1}{2}$ pounds) of bone and ten (22 pounds) of meat.

After the soup in the morning each person receives a ration of dry or fresh vegetables cooked with bacon.

After the evening soup, they receive the bacon which was used for cooking the vegetables in the morning.

The rations of fresh vegetables, such as potatoes, cabbages, carrots, and turnips weigh $37\frac{1}{2}$ grammes ($1\frac{1}{2}$ oz). The rations of cooked vegetables, such as haricots, peas, and lentils, $12\frac{1}{2}$ grammes. The rations of rice and millet 5 grammes.

The bones from which the gelatine is extracted come from the military hospital, the college, and the school. All the operations relative to this extraction are made in a room, separated from that in which the old men are, only by a wooden gate.

Before the introduction of the gelatine the regimen of St. Nicholas was exactly as at present; only the soup was prepared with hog's lard, salt, and spices.

It must be well understood that the new rule was not introduced for the sake of economy, the desire of improving the soup of the paupers was the only motive of the administrators. Every quarter of a litre of hog's lard broth comes to 0.92 centime; every quarter of a litre of animalized gelatine broth costs 1.25. centime.

The preceding details sufficiently show that the observations collected from the hospital, *St. Nicholas de Metz*, cannot decide *whether pure gelatine* is nutritive, but they may serve to appreciate the influence this substance exercises over animal economy, when it is mixed with bread, vegetables, and a very light meat broth.

The animalized gelatine broth has been *more than four years in use at the hospital St. Nicholas de Metz*. In these four years, from the unanimous testimony of the honourable directors of this establishment, the state of health of the five hundred persons it contains has evidently improved. The increase is found to be more than compensated by the decrease of expense for the infirmary.

M. Arago has received this information from M. Pedancet, counsellor to the royal court; from M. Prost, director of the fortifications of Metz, second commandant of the school of application, &c. &c.; and from M. Frecot.

The declarations that M. Arago has collected, by visiting the different halls of the hospital, have completely confirmed

the statement of the directors. With two or three exceptions in the section of old women, every one is pleased with the new regimen, and all say it is very superior to the old one, both as to taste and salubrity, and all wish it to be continued.

The military hospital at Metz used lately for the employés a preparation of gelatine which is now done away with. But M. Arago is assured by Dr. Scoulteten, that particular circumstances totally unconnected with the value of M. Darcet's process, are the only reasons for its temporary suspension. The employés are very well satisfied with the use of animalized gelatine broth, and would be glad to see it re-established.

M. Silvester wishes the communication of M. Arago to be inserted with detail in the *Compte Rendu*.

M. Magendie, on the contrary, regrets it has been made in the public session. He thinks M. Arago ought to have confined himself to sending *his conclusions* to the gelatine commission. This commission pursues with zeal and perseverance the duty that has been assigned to it, and in a short time will present its report. We shall then appreciate all the efforts it has made to overcome the difficulties of the question, and to escape the uncertainty with which complex experiments, such as M. Arago speaks of, abound. M. Magendie is of opinion that the gelatine might be abolished in the hospital *St. Nicholas de Metz* without the old men and children to whom it is given perceiving it, and certainly without their feeling any harm from it.

M. Arago answers that he has not presented *conclusions*, but only facts. The experiment at Metz, *considered*, if not physiologically, at least in an economical point of view, appears excellent even after the observations of M. Magendie. He doubts the commission having had the means of undertaking any thing of this sort so extensive, either as to duration or with regard to the number and diversity of the persons submitted to the gelatine regimen. In reply to the reproach of having addressed the academy in preference to the commission, M. Arago says he did it for the satisfaction of M. Darcet, who for the last seven years has been wanting to be relieved from the most uncomfortable position. The perpetual secretary adds that he will never hesitate, within the limits of right, justice, and truth, to render his professional friends all the services which are in his power.

M. Magendie declares the experiments on the nutritive properties of gelatine to have been commenced only two, and not seven years, ago.

M. Dumas adds that facts analogous to those which M. Arago has laid before the academy have not been disregarded

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by the commission : that he, for example, has had an opportunity of personally considering, on the spot, the results obtained at the hospital *St. Nicholas de Metz*.

XXI. *Note on the paralysis and neuralgie of the face.*
By M. MAGENDIE.*

The discussion which took place at the last session has reminded me of some medicinal facts, which, having a tendency to throw light upon the functions of the nerves of the face, I think worthy to be laid before the Academy.

I have lately paid attention to two young persons affected with hemiplegia of the face, a disease which at the same time compromises the receiving of the food, mastication, speech, the action of whistling, &c.; but which, above all, raising the expression of the figure, and rendering it completely immoveable, particularly when the passions are the most animated, renders the countenance something monstrous. If this paralysis were shown at the same time on both sides, the figure, that moving picture of our thoughts, would assume by its immoveableness a frightful aspect : it would be an inanimate mask on a living head.

This state is fortunately very rare, and not even met with in the case of general paralysis, where every kind of motion of the body and limbs is stopped : by the goodness of providence there remains to the patient his phisiognomy to express his sufferings or desires.

I have had the good fortune to cure these paralyzes of the face by means of electricity applied to the nerves themselves, with the assistance of platina needles. In the course of these applications I had the opportunity of remarking that the nerve became perfectly incapable of exciting the contractions of the muscles, preserving however a sensibility which apparently differed nothing from the sensibility it possessed in its healthy condition, which supports the results of my last experiments ; proving that the sensibility of the facial nerve is only apparent, being no other than that of the threads of the nerves of the fifth pair which intermix with the threads of the facial nerve.

There is nothing, in this sensibility, existing in a paralyzed motive nerve which ought to surprise us now, though for a long time I was much perplexed by it.

* From the *Comptes Rendus*, June 17, 1839. Translated by Mr. J. H. Lang.

It ought, theoretically, not only to exist in the trunk, but in all the branches. It is this also which exists, and which I have several times mentioned, in the two diseases I have just spoken of.

M. Roux, mentioning a neuralgie case in which he cut all the facial branches in succession, and perceived the pain take refuge successively in the uncut branches, has turned my attention to these very mysterious relations which are seen among the branches of the same nerve.

I saw a very remarkable instance of this last month.

A middle aged woman came to me to be cured of a most intense neuralgie which for five years had given her no relaxation, and had entirely deprived her of sleep.

This neuralgie did not occupy every branch of the fifth pair at once, but was sometimes in one, sometimes in the other, and always with equal violence; the place varied but the pain was the same.

The day she came to me the disease was seated in the right lower maxillary; I applied the electric current and the pain immediately passed to the tongue, leaving the maxillary nerve. I forced a needle in the right side of the tongue, and caused the current to pass over it; the pain entered the under-orbitary. I also followed it there. It went at last to the frontal nerve, at which it was not difficult to arrive: I attacked it there in the same manner, and it disappeared; fortunately not having taken, as a last resource, the nasal thread of the opthalmic, as in the case quoted by M. Roux; but which, however, would not have entirely discouraged me, for, by the assistance of a fine platina needle, I do not consider impossible to have got at it; and, consequently, to have directed an electric current to it.

My patient, to her great satisfaction, was thus relieved from a disease which had been the torture of her existence. The disease has since made some attempts to return; but weak and very tolerable: above all, never preventing sleep: and a single application of the electricity causes it immediately to disappear.

For some days I have had an opportunity of seeing a Parisian lady, who has also been troubled for some time with a neuralgie; but nearly always situated in the right lingual nerve. I applied the electricity, and the pain passed into the upper maxillary nerve; from which I immediately removed it. It went no further, and thus saved me the trouble of pursuing it.

How are these transmissions made? Is it by the branches themselves? or is it by those numerous anastomoses; little

known to any but professional anatomists? I am not at present able to obtain any well founded opinion as to this curious question.

XXII. *Observations on the means to be adopted for ascertaining the temperature of vegetables.* By M. BECQUEREL.*

As soon as I had applied thermo-electric effects to determine the temperature of the interior parts of men and animals, I tried the same sort of experiment to ascertain that of vegetables. The general process consists in using two perfectly similar needles, each composed of one steel and one copper needle soldered together at one end, while the free ends are thus connected: the steel ends with a wire of the same metal, and the copper with the two extremities of the wire which forms the circuit of a multiplier. When the temperature is the same at the two soldered parts, the magnetized needle remains in equilibrio; but if there be only the difference of a tenth of a degree, it is indicated by the deviation of the needle. Hence, by keeping one of the soldered parts at a constant and known temperature, we may, by means of a table which gives the relations between the deviation of the needle and the difference of temperature, ascertain that of the other soldered part which is variable.

In men and animals, whose interior temperature is much above that of the surrounding air, we place one of the soldered parts in an apparatus having a constant temperature of 38 degrees, and the other by means of puncturation is admitted to the part we wish to explore.

When I wished to apply this process to vegetables, I immediately saw the impossibility of using an apparatus of a constant temperature, on account of the slight difference existing between the temperature of the air and that of vegetables: the apparatus used for animals requiring a difference of a certain number of degrees to be employed to advantage.

Being anxious, however, to resolve the question with regard to vegetables, as I had done in conjunction with M. Breschet that respecting animals, I requested M. de Mirbel, two years ago, to join me in making experiments at the "Jardin des Plants." He accepted my proposition, and immediately placed at my disposal those shrubs which were likely to be

* From the *Comptes Rendus*, June 17, 1839. Translated by Mr. J. H. Lang.

most useful to us. I soon perceived the difficulties I had to overcome in attaining the desired end. We commenced by piercing one of the shrubs with a very delicate gimlet, so as to introduce one of the solders. The needle introduced was soon changed, which produced an electro-chemical current. To remedy this evil the needles were covered with several coats of gum-lac varnish; the other solder remained in the air, where the temperature was, to all appearances, constant; but the radiation not being the same at the two extremities, the one being covered with the ligneous tissue, and the other in the open air, there resulted very complicated effects which required to be resolved, as, otherwise, we could have no means of ascertaining the temperature of the vegetables. M. de Mirbel proposed that I should operate in the middle of the garden, placing the apparatus in one of the gardeners' cabins. I accepted his offer, and on entering perceived a tree in full vegetation (an acacia I believe), and at the side a branch of the same tree. It immediately struck me, that to avoid the difference of radiation which was an obstacle to the success of our experiments, we might put one of the solders in the living and the other in the dead branch of the same tree, both having apparently the same diameter. This experiment which was indicated by theory succeeded perfectly; and we soon perceived a difference between the temperature of the living and that of the dead tree. The gardener was commissioned to note the deviations every two hours; but I perceived the next day that notwithstanding his intelligence he had made so many errors in the observations, that I was obliged to give it up; intending to return to it when I had finished some labours of another kind which I had already begun.

The next year M. Dutochet asked me for some instructions on the means to be employed for determining the temperature of vegetables: I told him all I had done on this subject, requesting him to make use of my process which might conduct him to the solution of the question. It is with the greatest pleasure I see that he has made use of them, and the observations already collected will assist the process of vegetable physiology. I do not doubt but the memoir he is about to publish will contain only the details I have just communicated to the Academy; and for which there was no room in the short paper read during the last session.

XXIII. *Electricity. Extract of a letter from M. PELTIER, in reply to an objection of M. PARROT.**

The objection of M. Parrot having been only indicated in the *Compte Rendu*, we shall only announce the reply of M. Peltier. The letter of this latter philosopher also contains some considerations, independent of this discussion, and these we shall here mention.

I have shown in former communications, says M. Peltier, that the *quantity* of dynamic electricity is proportional to the quantity of molecules whose equilibrium has been disturbed. I add that the duration of the electric phenomena is equal to that of the passage from the old to the new equilibrium, and that there is a connexion and dependence between these two states.

I have also stated, that the currents of induction increase as the magnetic intensity of the bars; I have since found that it was the same with the caloric, when placed in the shade of the neutralization in return for the quantities we wish to measure. It now remains to discover whether the augmentation of the disturbing force, that is to say, whether a more intense magnetism or a greater elevation of temperature, would really give a more copious current; or whether, on the contrary, the phenomenon of *quantity* was not the product of the electric *intensity*, from this power overcoming resistances, which otherwise would oblige more electricity to be neutralized by the direct circuit. To arrive at the complete solution of this question, we must have measuring arcs, of a perfect conductivity, which it is not possible for us to obtain; but by employing very short circuits, three or four decimeters at most, we can show that the first degree of heat gives a greater deviation than the second, the second than the third, and so on; that the difference in favour of the first degree is as much greater as the resistance of the circuit is less; and, consequently, if the conductivity be perfect, the first degree will give the maximum current in the thermo-electric circuit. This proves also that the intensity of the disturbing force does not increase the *quantity* of dynamic electricity, but gives to the quantity produced more of that power which we call *intensity*. The result of this experiment is, that the causes which do not change the nature of the bodies, as heat and induction, act like piles, in which the *intensity* increases as the number of pairs, while the *quantity* remains the same.

* From the *Comptes Rendus*, June 17, 1837. Translated by Mr. J. H. Lang.

This similitude of effect gives the idea, that there is between the atoms thus disturbed, as between the pairs of a pile, a continuation of the neutralization of the two electric states; and which leave free only the two extreme states, positive on the one side, and negative on the other, which are neutralized over the interposed conductor.

This analogy does not exist when the force of preturbation changes the nature of the substances, as, for example, chemical action. The atom which combines immediately, ceases to form part of the body; there is no longer solidity between them; each molecule produces its electric phenomenon insulated and complete, which has no influence over the phenomenon produced by the neighbouring molecule; thus the intensity remains the same with a feeble or strong reaction, the *quantity* varies only because there is a greater transformation in a given time.

It is a very remarkable particularity, that there is no corresponding ratio between the temperature and the current. We have just said that a double temperature gives the dynamic electricity a double *intensity* of power; but when a current traverses a metallic wire, and it is made to increase as two to one of its powers, the temperature of the wire increases as three to one of its powers.

XXIV. *Letter from M. BREGNOT, inspector of buildings to the "Hôtel des Invalides," to M. ARAGO, on the thunder clap which struck the dome, June 8th, 1839.**

I have the honour to enclose you some particulars, corrected from those very inexact ones, sent to the Académie de Sciences, by M. Leymerie, of the effects of the storm, of Saturday, June 8, on the Royal "Hôtel des Invalides."

It is true the thunderbolt fell on the dome des Invalides, and that in consequence of the fracture of one of the metallic chains serving as a conductor, it was precipitated into a small adjoining court: but here stops the correctness of the details forwarded by M. Leymerie.

The chain was perfectly whole and very solid, six days before the event. It was visited on the 2d of June, by the officers of the buildings; and the fracture being about the height of the hand, could not have missed being seen.

I am certain that it was broken by the thunderbolt itself, both from the attentive examination made on the 2d of June, and from the observation made immediately after the storm. We discovered that it was not a simple solution of continuity,

* From the Comptes Rendus, June 17, 1839. Translated by Mr. J. H. Lang.

which would have been the result of the oxidation of the metal, but that a length of at least thirty centimeters (about 12 inches) had been broken and dispersed into an infinite number of pieces of equal lengths, not exceeding forty millimeters ($1\frac{1}{2}$ inch).

This chain is composed of about twenty iron wires twisted together. At the parts in which it was broken it passes through an iron ring or collar, fixed in the wall; and, without doubt, to diminish the effort of its weight, it was twisted twice round this collar. This arrangement which I think bad, added to the angular direction the chain takes at this place, appears to me to be the cause of the fracture. However, these things have been the same from the establishment of the lightning conductors; and nothing of the kind has ever happened before.

The following are the other effects of the electric discharge:—

1. About sixty nails, fixing the gilt lead about the lantern of the dome, on a circumference of about twenty meters,* and at least fifty meters above the spot where the chain was broken, were violently torn out and dispersed in all directions.

2. The lead was raised by the displacement of these nails, and formed projections from thirty to forty millimeters.†

3. The lead surrounding the base of one of the columns of the lantern was so torn off that we could not find the least vestige of it; yet there was no sign of fusion or vitrification.

4. After the rupture of the conducting chain, the thunderbolt struck a small building in which are the bath rooms; it broke into four pieces, a stone, about 1^m.25 high, 0.30 wide, and weighing 77 kilogrammes (190lbs.), which it hurled 4 meters from the wall against which it was leaning.

It is remarkable that this stone was not attached to the wall by a metallic connexion, but it hid the orifice of a leaden pipe, used for discharging the water from the reservoir of the baths.

5. On the right, as you face this stone, is a door leading into the bath rooms. This door was open at the time the thunderbolt fell; and although the place contained twenty copper bath tubs, cocks, pipes, and pump apparatus, the lightning did not enter it. The bath keeper was standing at this door and felt nothing but a very natural fright, as he was not 50 centimeters from the stone which was struck.

6. On the right (looking at the door), is a window defended by a square iron grating. The four great squares of this window were broken in pieces; the largest of which was scarcely two centimeters wide.

* Each metre is upwards of 39 inches.

† Each millimeter is upwards of .0397 of an inch.

• 7. Several other squares were broken ; but I think this ought to be attributed more to the shock than any regular action.

8. Finally, on the roof of this building, above the entrance door of the bath room, several of the slates were pierced with holes as if there had been a discharge of grape shot.

These are the details of the effects produced by this storm. My relation, though rather long, if it have no other merits, has that of being exact and conscientious. The assertion of a solution in the continuity of the conductor previous to the storm, and the testimony brought to support it, are not worthy of belief.

We shall return to the thunderbolt of the Invalides, and the consequences likely to flow from it, as soon as we have received different documents of which we are in expectation.

XXV. *Account of a Tornado, which, towards the end of August, 1833, passed over the city of Providence, in the state of Rhode Island, and afterwards over a part of the Village of Somerset. Also an Extract of a Letter on the same subject from Zachariah Allen, Esq., of the city of Providence.**

Communicated by Robert Hare, M. D., Professor of Chemistry in the University of Pennsylvania. Read October 28, 1838.

I propose to lay before the Society, for a place in their Transactions, an account of a tornado which occurred in the state of Rhode Island, towards the end of August last.

This phenomenon was first observed near Providence, over the south western suburbs which it passed in a course generally from west by north, to south by east. Only a few days subsequently I visited some of the most remarkable scenes of its ravages.

The characteristics of this tornado, from all that I could see or hear, are quite similar to those of the tornado which occurred at New Brunswick, New Jersey, in June, 1835, and to which I referred in my paper upon the causes of tornadoes and water-spouts, published in the sixth volume of the Society's Transactions.†

This recent tornado was advantageously seen by J. L. Tillinghast, Esq., from a window of his mansion, which is so situated, on the brow of a hill on the eastern side of the city of Providence, as to afford an unobstructed view of the country opposite. Mr. Tillinghast alleges that his attention was

* From the Transactions of the American Philosophical Society at Philadelphia.

† The New Brunswick Tornado is described at p. 195, Vol. II. of these Annals.

at first attracted by seeing to the westward a huge inverted cone, of extremely dark vapour, which extended from the clouds to the earth. In the contortions and spiral movements of its lower extremity, this cone was conceived to resemble the proboscis of an enormous elephant, moving about in search of food. Sometimes it was elongated so as to reach the ground; at others it skipped over the intervening space without touching it; but at each contact with the terrestrial surface, or bodies resting thereon, a cloud of dust, intermingled with their fragments, was seen to rise within the vortex. To those who were sufficiently near to the meteor, a fearful explanation of these appearances was simultaneously evident. Ponds were partially exhausted. Trees uprooted or deprived of their leaves or branches. Houses were unroofed, or uplifted and then dashed to pieces. Farms were robbed of their grain, potatoes, fruit-trees, or poultry: nor were human beings secure from being carried aloft, and more or less injured by subsequent descent. It was alleged that at Somerset two women were carried from a waggon over a wall, into an adjoining field. Within the same village a cellar door frame, with its doors bolted, was lifted, and then deposited on one side of its previous position; although situated to windward of the mansion to which it belonged. This result was the more striking, because, in consequence of their presenting an inclined plane to the blast, the doors and their frames would have been pressed more firmly upon their foundation by an ordinary wind. In consequence of the same dilatation of the air within the house, which lifted the cellar door, the weatherboarding on the leeward side was burst open, while that to the windward was undisturbed.

About four o'clock on the afternoon during which this tornado passed near Providence, there was heard at the farm at which I resided, twenty-five miles south of Providence and about fifteen miles from Somerset, the loudest thunder which I ever heard. It made the house in which I was tremble sensibly.

I have received from an estimable friend, Mr. Allen, a most interesting account of this tornado, which passed over the river, and there produced the appearance of a water-spout, while he was sufficiently near for accurate observation. In one respect his narrative tends to justify my opinion, that the exciting cause of tornadoes is electrical attraction. In two instances in which flashes of lightning proceeded from the water, Mr. Allen remarked that the effervescence produced by the tornado in the water very perceptibly subsided.*

* See *Essay on the Cause of Tornadoes or Water-spouts* in sixth vol. *American Philosophical Transactions*, or in *Silliman's Journal*, vol. 32, for 1837, and at p. 195, Vol. II. of these Annals.

*Extract from a Letter written by Zachariah Allen, Esq.,
of Providence.*

"It was about three o'clock, P. M., during a violent shower, that I observed a peculiarly black cloud to form in the midst of light, fleecy clouds, and to assume a portentous appearance in the heavens, having a long, dark, tapering cone of vapour extending from it to the surface of the earth. The form of this black cloud, and of the cone of vapour depending from it so nearly resembled the engraved pictures of 'water-spouts' above the ocean, which I had frequently seen, that I should have come speedily to the conclusion that one of these 'water-spouts' was approaching, had I not been aware that this phenomenon occupied a space in the heavens directly above a dry plain of land. Whilst attentively watching the progress of the cloud, with its portentous dark cone trailing its point in contact with the surface of the earth, I noticed numerous black specks, resembling flocks of blackbirds on the wing, diverging from the under surface of the clouds, at a great elevation in the air, and falling to the ground. Among these were some objects of larger size, which I could discern to be fragments of boards, sailing off obliquely in their descent. This alarming indication left no room for doubt that a violent tornado was fast approaching, and that these distant, dark specks were fragments of shingles and boards uplifted high in the air, and left to fall, from the outer edge of the black conical cloud. This fearful appearance was repeatedly exhibited, as often as the tornado passed over buildings.

"The whirlwind soon swept towards an extensive range of buildings, within a few yards of me, the roof of which appeared to open at the top, and to be uplifted for a moment. The whole fabric then sunk into a confused mass of moving rubbish, and became indistinctly visible amid the cloud that overspread it, as with a mantle of mist.

"The destructive force of the tornado now became not only apparent to the eye, but also fearfully terrific, from the deafening crash of breaking boards and timbers, startling the amazed spectator in alarm for his personal safety, amid the roar of the whirlwind, and the shattered fragments flying like deadly missiles near him. At one instant, when the point of the dark cone of cloud passed over the prostrate wreck of the building, the fragments seemed to be upheaved, as if by the explosion of gunpowder, and I actually became intensely excited with the fear that the moving mass might direct its march toward the open area of the yard, to which I had resorted, after abandoning a building in which I had previously found shelter.

“Fortunately the course of the tornado was not over the building used as a depot by the Stonington Railroad Company in Providence, where there was a numerous assemblage of passengers awaiting the departure of the cars; otherwise several lives might have been lost.

“The most interesting appearance was exhibited when the tornado left the shore, and struck the surface of the adjacent river. Being within a few yards of this spot, I had an opportunity of accurately noting the effects produced on the surface of the water.

“The circle formed by the tornado on the foaming water was about three hundred feet in diameter. Within this circle the water appeared to be in commotion, like that in a huge boiling cauldron; and misty vapours, resembling steam, rapidly arose from the surface, and entering the whirling vortex, at times veiled from sight the centre of the circle, and the lower extremity of the overhanging cone of dark vapour. Amid all the agitation of the water and the air about it, this cone continued unbroken, although it swerved and swung around, with a movement resembling that of the trunk of an elephant whilst that animal is in the act of depressing it to the ground to pick up some minute object. In truth, the tapering form, as well as the vibrating movements of the extremity of this cone of vapour, bore a striking resemblance to those of the trunk of that great animal.

“Whilst passing off over the water, a distant view of the cloud might have induced the spectator to compare its form to that of a huge umbrella suspended in the heavens, with the column of vapour representing the handle, descending and dipping into the foam of the billows. The waves heaved and swelled, whenever the point of this cone passed over them, apparently as if some magical spell were acting upon them by the effect of enchantment. *Twice I noticed a gleam of lightning, or of electric fluid, to dart through the column of vapour, which served as a conductor for it to ascend from the water to the cloud. After the flash the foam of the water seemed immediately to diminish for a moment, as if the discharge of the electric fluid had served to calm the excitement on its agitated surface.*

“The progress of the tornado was nearly in a straight line, following the direction of the wind, with a velocity of perhaps eight or ten miles per hour.

“Near as I was to the exterior edge of the circle of the tornado, I felt no extraordinary gust of wind; but noticed that the breeze continued to blow uninterruptedly from the same quarter from which it prevailed before the tornado occurred.

"I also particularly observed that there was no perceptible increase of temperature of the air adjacent to the edge of the whirlwind, which might have caused an ascending current by a rarefaction of a portion of the atmosphere. After passing over the sheet of water, and gaining the shore, I observed the shingles and fragments of a barn to be elevated and dispersed high in the air; and the dark cloud continued to maintain the same appearance which it at first presented, until it passed away beyond the scope of a distinct vision of its misty outlines.

"The above imperfect sketch can convey to your mind only a feeble impression of this exciting scene, which in passing before me excited just enough of terror to impart to the spectacle the most awful sense of the power, sublimity, and grandeur of the Almighty, as described in the glowing words of the Psalmist. 'He bowed the heavens also, and came down; and darkness was under his feet; and he did fly upon the wings of the wind. He made darkness his secret place; his pavilion round about him were dark waters and thick clouds of the skies.'"

XXVI. *On various Electrical Apparatus, &c.* By CHARLES BARKER, ESQ. *In a letter to the Editor.*

Sir,

Should the following remarks be worth a place in your next number, you will perhaps oblige me so far as to insert them, first :—

With regard to the Leyden Jar,

I have for some time past adopted a plan which answers well. I pass the tube, communicating with the inside coating, through a *glass* tube, extending above and below the cover about six inches. The cover is thus insulated from the inside coating; whilst, at the same time, I have the advantage of the stability which it gives the wire, and also the exclusion of all dust. My battery will thus retain its charge for days: thanks, however, in some measure, to Dr. Arnott. Fig 7, Plate V, will show the plan.

The Universal Discharger.

I have made a little improvement in this instrument. As at present made you cannot introduce anything between its arms wider than the distance will admit of; but I cause that part

of the stand, which supports the insulated discharging arms, to slide out as shown in fig 8. It is very convenient in many respects.

The revolving Spotted Fly.

In Number 2 of the Annals you describe "a brilliant experiment for the lecture table," and which I find the opticians make for the moderate sum of five pounds. With the permission of my friend, Mr. Nicholson, of Fareham, I can describe a better one, and which may be made for less than as many pence, and which will work, I can confidently say, much better. Mr. Nicholson invented it six or seven years ago; and I then mentioned it to the leading opticians in town: amongst whom I may mention Messrs Jones, of Holborn, who made it to my order for seven shillings. I am thus particular because I had just seen a book called the "*Experimental Philosopher*," in which the experiment is mentioned and claimed by the author, Mr. Mullinger Higgins, as his. It is true that at page 319 of his work, he says, that it has been described to him as my invention, but this comes *after* the paragraph suggesting it as his own. If the experiment is really original with him, it is certainly curious to remark the singular similarity between them. When my friend, Mr. Nicholson, first made the experiment nearly, or quite, seven years ago, he placed it horizontally; but when made vertically, it is seen much better on the lecture table, as shown in fig. 9; which will explain the way of showing it. I may just observe that it works much better if placed on an *insulated stand* and thus driven by sparks instead of being directly connected with the conductor. The same plan of the insulated stand, answers also much better for exhibiting the common electrical fly; which if thus placed, and the *points greased*, as mentioned, I think, by Singer, forms a very pretty experiment, as you thus get a *dotted circle of light*; and by varying the length of the sparks, you may diversify the experiment very well. I can assure you it is well worth trying. The instrument is represented by fig. 9.

Revolving Spiral Tube.

Fig. 10 represents my improved plan of the revolving spiral tube. It is made of a glass tube blown nice and round at one end, and open at the other; ten inches long, and three-quarters diameter. A ball or a piece of smooth tinfoil is fixed at the upper closed end, and the usual spots of tinfoil carried in a spiral form to the lower open end. A wooden ring is cemented on the outside of the lower end of the tube

and a strip of foil pasted round it. From this ring, and touching the tinfoil, four wires project outwards, having their points bent at right angles in the usual way. The tube is then set on an upright wire which passes upwards into the tube to its top, and this wire is then set on an insulated stand. It can thus revolve with great ease; the expense is a mere trifle; and it answers well.

The Spotted Jar.

In the Annals, for June, 1838, a correspondent asks "how is it when a jar is coated in the usual way on the inside, and spotted on the outside, you do not see the sparks on its outside when charging?" I have tried the experiment and can only say that, as I had anticipated, I did see the sparks passing as usual.

The Falling Star.

In the Annals, for April, 1837, a correspondent requests information as to this experiment. The principal requisites are to have the jar *intensely* charged, and the tube exhausted to a certain extent. My tube is six feet long, and four inches diameter, and I use a jar having five square feet of coating. A friend works the air pump, whilst I occasionally try to pass the charge down, when at a certain degree of exhaustion it does so, in a most brilliant line of white light. In order to avoid the injury of the sharpe edged cylindrical caps, which throw off the electric fluid, in this and similar electric experiments, I have all my apparatus fitted with wooden rings, which cover such edges.

On the identity between Electricity and the Aurora.

When electricity is passed down a perfectly exhausted tube it presents the appearance, and is a close imitation, of the aurora; and is, I believe, supposed to be one and the same thing. As the aurora is only electricity on a larger scale, passing through an attenuated atmosphere, they should produce similar effects. Now, the aurora deflects the magnetic needle; and the passage of electricity down a vacuum should do the same. I take a glass tube three feet long, and two inches diameter, mounted with caps and a valve, as usual, so that it may be exhausted. Round the centre of this tube, and ten inches from either end, I wind one hundred and eighty feet of covered copper wire; the two final extremities of which are connected with a galvanometer: see fig. 11. The tube is insulated, and has one end connected with the outer coating of a battery, of fifty square feet; and the other end, with an insulated discharger, by means of a long wet thread. The

battery being charged and the tube exhausted, I connect one end, by means of the wet thread, with the battery, for the space of five seconds, then remove for the same time, then again apply it, and so on in the manner mentioned by Dr. Faraday, in his Researches. The needle is soon deflected nearly 30° . I believe this is new.

Now, allow me to make a few remarks on the common electric machine. It is generally admitted that the plate is more powerful than the cylinder; but as the plate is generally inverted you cannot obtain the negative electricity, so that in this respect the cylinder is preferable. When, however, the plate is inverted on the plan of Mr. Harris, of Plymouth, you can readily obtain both positive and negative electricity. It is both powerful in action and elegant in appearance; and yet we see works on the subject constantly appearing, in which the writers say the best machine, &c. for the experimentalist, is the cylinder. My electrical machine, constructed by Watkins and Hill, is a double three feet plate on Harris's plan, and I can safely say that its effects are most powerful, far more than any I ever yet saw. One turn will give from fifteen to eighteen sparks one inch long, passing between balls one inch and a quarter diameter. It has four pair of rubbers; and I will venture to say, that no one will readily take two sparks from the conductor.

Can you inform me how to coat the inside of large carboys, so as to make them Leyden jars?*

Your obliged,
CHARLES BARKER.

Gosport, July, 8, 1839.

* The method which I have employed to line narrow necked bottles with metal is similar to that of silvering the inside of glass globes for mirrors. Melt equal parts of lead and tin, and whilst fused add quicksilver enough to keep the whole fluid whilst warm; and in this condition pour it into the bottle or carboy, turning the latter round and round in various ways till the whole of the inside be covered with the amalgam.

A little bismuth added to the other metals keeps the whole fluid at a low temperature, which is an advantage. This is the best method with which I am acquainted. The bottles may be lined up to the orifice of the neck. Metallic filings never do well for lining an electrical jar. I think that I first saw this plan practised by Mr. Marsh. EDIT.

XXVII. *Fine Arts.—The Daguerre Secret.*

At the weekly sitting of the Academy of Sciences, on Monday, Aug. 19, the process of M. Daguerre, for the formation of photogenic drawings, was, as had been previously announced, communicated to the public. From an early hour all the seats allotted to the public were occupied; and upwards of 200 persons, disappointed of gaining admission, were stationed in a crowd in a court of the Institute, and formed a kind of scientific *émeute*. Every body was anxious to hear *the* secret, every body to catch the *mot*; all were desirous of learning whether their own scientific conjectures would be confirmed or not.

At three o'clock, M. Arago commenced his explanation. We shall not attempt to follow the great *savant* through all the details of his long statement, a large proportion of which related to the history of the discovery,—a subject already pretty well known to the public: and we shall only mention as much of that part of it as belongs to Messrs. Niepce and Daguerre. In a similar manner, our account of the process itself will be brief: both because M. Arago was obliged to give only a *résumé* of it, and because it is announced that, in a few days' time the documents communicated by M. Daguerre, under seal to the Committee of the Deputies, on his grant, will be published, and the inventor himself will give a series of public representations of the actual performance of his method.

M. Arago, after alluding to the history of the camera obscura, originally discovered by Porta, a Neapolitan chemist, reminded the Academy that, as early as 1566, the influence of light on what the alchemists termed *lune* or *argent corné* (chlorure of silver), had been observed, and was mentioned in the work of Frabicius. A Frenchman, named Charles, at the commencement of the present century, had made use of a sensitive paper for the tracing of outlines, by the action of light, but had died without leaving any account of his method. After this came the memoir of Wedgwood, of 1802, &c.; a part of the history of the discovery too well known to need repetition. The late M. Niepce, M. Arago proceeded to state, was living near Châlons-sur-Saône, occupied in scientific pursuits, and appeared to have commenced his photographic experiments in 1814. His first connexion with M. Daguerre commenced in 1826, when, through information received from an optician at Paris, he learned that this gentleman was engaged in an independent series of photogenic researches, and especially in trying to fix the images of the

VOL. IV.—No. 21, October, 1839. R

camera obscura. They associated their labours in 1829, after M. Niepce had visited London in 1828, and had presented his memoir on his photogenic discoveries to the Royal Society. It is proved that for the *photographic copying* of engravings, and for the formation of plates for engravers, in an advanced state of preparatory sketching of the subject, this gentleman was, in 1826, possessed of the secret of making shades correspond to shades, light parts to lights, demi-tints to demi-tints, &c.; and that he also knew how to make his drawings so produced insensible to the ulterior action of light. The act of partnership, drawn up between M. Niepce and M. Daguerre, and which afterwards stood good between his son and the latter gentleman, states that some entirely new methods had been discovered by M. Daguerre, and that they had the advantage of being able to reproduce images from sixty to eighty times more rapidly than by the processes previously adopted.

It appears that M. Niepce, in his own photographic researches, had first made use of a sheet of silver covered with the purest bitumen (called, in France, *Bitume de Judée*), which had been previously dissolved in oil of lavender. He used to heat this sheet of silver till the oil was completely evaporated, and only a kind of whitish powder remained adhering to the surface. He then placed the sheets so prepared in the focus of the camera obscura, and obtained the image of the object; but the trace was hardly visible. To obviate this imperfection, M. Niepce next thought of plunging the sheet, when removed from the camera obscura, into a mixture of oil of lavender and oil of petroleum,—a method that succeeded and augured still further improvement; for the image became visible like an ordinary engraving, and on washing the sheet with distilled water, left the representation permanent. As a further improvement of this, M. Niepce used a new mixture of sulphuret of potassium and iodine; but the light acted very slowly upon it, and the iodine, spreading itself over the surface, rendered the image confused and obscure. The oil of petroleum, first used by this gentleman, was found to have the property of attaching those points of the metallic surface, which had been preserved from the action of light by the shades, while it was of no effect on those parts touched by the solar rays.

It was at this stage of the invention that M. Daguerre's labours were joined to those of Niepce; and it is only after a long series of experiments, carried on with unwearied perseverance for many years, that, after M. Niepce's death, M. Daguerre has at length resolved the problem to its present extent. We omit the history of these experiments, and pass

on at once to the actual process as is now used by its author.

A sheet of copper, plated with silver, is washed carefully in a solution of nitric acid, which removes from it all the extraneous matters on its surface, and especially any traces of copper from the silver surface. A slight degree of friction is requisite in this process, but it must not be applied always in the same direction. M. Daguerre has observed, that with the friction used in a particular manner, a sheet of copper thus plated with silver answered better than a sheet of silver alone; and he infers from this, that voltaic agency is not unconnected with the effect. When the sheet is thus prepared, it is placed in a closed box and exposed to the vapour of iodine. This vapour is made to pass through a very fine sheet of gauze, to render its distribution more equable over the surface of the silver, and in order to effect this object (which is quite indispensable) more certainly, the sheet has a small metallic rim raised round all its edges. A thin coating, of a yellow colour, is thus formed on the surface of the sheet, which is estimated by M. Dumas at not more than the *millionth part of a millimetre* in thickness. The sheet, when covered with this substance, is of the most excessive sensibility to light; and is thus ready for the camera obscura. M. Daguerre, in the instrument which he uses, employs a piece of unpolished glass, which he brings first of all into the focus of the lens, in order to determine the exact point at which the sheet should be placed; and, as soon as this is determined, the sheet is placed accordingly. A few seconds, or minutes, according to the time of day, the state of the atmosphere, &c., suffice for forming the photogenic image; but it is hardly, if it all visible on the surface of the sheet. To make it so, the sheet is placed in another box, and exposed to the vapour of mercury, heated at 60° Reaumur, or 167° of Fahrenheit. One of the most curious circumstances attending this part of the process is, that the mercury must act at a certain angle. If the drawing is intended to be seen vertically, it must be suspended over the mercury at an angle of 45° ; if it is to be seen at an angle of 45° , it must be suspended horizontally. On being taken out from the mercury-bath, the sheet is plunged into another bath of the hypo-sulphite of soda; this solution attacking the parts upon which the light has not been able to act, and respecting the light parts,—being the very inverse of the action of the mercury. It may be supposed, therefore, observed M. Arago, that the light parts of the image are formed by an amalgamation of mercury and silver, and the dark parts by a sulphuret of silver, at the expense of the hypo-sulphite of soda. M. Arago observed, that no satis-

factory reason had yet been given for this latter part of the process. The sheet is finally washed into distilled water, and the operation is terminated.

The drawing, thus obtained, is perfectly insensible to the action of light, but it is liable to injury, just like a crayon or pencil-drawing, and requires to be preserved under a glass. The effect of the whole is miraculous: and as an instance, we may mention that one of the drawings exhibited by M. Daguerre, on Monday, was the view of a room with some rich pieces of carpet in it; the *threads* of the carpet were given with mathematical accuracy, and with a richness of effect that was quite marvelous.

Such is the process of M. Daguerre; we must add, that it is found that the sun-light does not act equally well at all hours of the day, nor even when the sun is at equal heights above the horizon. Thus the effect is produced better at ten in the morning than at two in the afternoon; and hence the *Daguerrotype* may be of immense value in measuring the intensity of light.

The camera obscura, employed by M. Daguerre, may be put in a box two feet long, two feet wide, and two and a half feet high: the price of the whole apparatus may be from 400 francs to 420 francs; the price of each metallic sheet is about three or four francs.

M. Daguerre has intrusted the manufacture and selling of the apparatus, to the house of Alphonsa Giroux, in the Rue de Coq, St. Honoré.

We need hardly say that the most enthusiastic cheers responded from the grave benches even of the academy, on the termination of M. Arago's description; and the President, M. Chevreul, complimented M. Daguerre in the warmest terms.*—*Literary Gazette*.

* It seems to us that, beautiful as this process is upon metallic substances, much of the utility of photogenic copying will be lost, unless the artist and traveller can use paper instead of copper, *silvered, iodined, and mercurialised*. The images produced by M. Daguerre are exquisitely correct, but gloomy-looking. They resemble moonlight pictures done in ink. We also hear from Paris, that M. Collat has succeeded in his method of copying busts, statues, or other solid objects, with mathematical precision. This is, perhaps, as remarkable a discovery as the photogenic; and one that may be applied to many valuable purposes.—*Ed. L. G.*

XXVIII. *Supplementary Note to Dr. FARADAY'S Eleventh Series of Experimental Researches.*

Received March 25th, 1838.

1307. I have recently put into an experimental form that general statement of the question of *specific inductive capacity* which is given at No. 1252 of Series XI, and the result is such as to lead me to hope that the Council will authorize its addition to the paper in the form of a supplementary note. Three circular brass plates, about five inches in diameter, were mounted side by side upon insulated pillars; the middle one, A, was a fixture, but the outer plates B and C were moveable on slides, so that all three could be brought with their sides almost into contact, or separated to any required distance. Two gold leaves were suspended in a glass jar from insulated wires: one of the outer plates B, was connected with one of the gold leaves, and the other outer plate with the other leaf. The outer plates B and C were adjusted at the distance of an inch and a quarter from the middle plate A, and the gold leaves were fixed at two inches apart; A was then slightly charged with electricity, and the plates B and C, with their gold leaves, thrown out of insulation *at the same time*, and then left insulated. In this state of things A was charged positive inductrically, and B with C negative inducteously; the same dielectric, air, being in the two intervals, and the gold leaves hanging, of course, parallel to each other in a relatively unelectrified state.

1308. A plate of shell lac, three quarters of an inch in thickness, and four inches square, suspended by clean white silk thread, was very carefully deprived of all charge (1203.), so that it produced no effect on the gold leaves if A were uncharged, and then introduced between plates A and B; the electric relation of the three plates was immediately altered, and the gold leaves attracted each other. On removing the shell lac this attraction ceased; on introducing it between A and C it was renewed; on removing it the attraction again ceased; and the shell lac when examined by a delicate Coulomb electrometer was still without charge.

1309. As A was positive, B and C were of course negative; but as the specific inductive capacity of shell lac is about thrice that of air (1270.), it was expected that when the lac was introduced between A and B, A would induce more towards B than towards C; and therefore B would become more negative than before towards A, and, consequently, because of its insulated condition, be positive externally, as at its back or at the gold leaves; whilst C would be less negative towards

A, and therefore negative outwards or at the gold leaves. This was found to be the case; for on whichever side of A the shell lac was introduced the external plate at that side was positive, and the external plate on the other side negative towards each other, and also to uninsulated external bodies.

1310. On employing a plate of sulphur instead of shell lac, the same results were obtained; consistent with the conclusions drawn regarding the high specific inductive capacity of that body already given (1276.).

1311. These effects of specific inductive capacity can be exalted in various ways, and it is this capability which makes the great value of the apparatus. Thus I introduced the shell lac between A and B, and then for a moment connected B and C, uninsulated them, and finally left them in the insulated state; the gold leaves were, of course, hanging parallel to each other. On removing the shell lac the gold leaves attracted each other; on introducing the shell lac between A and C this attraction was increased (as had been anticipated from theory), and the leaves came together, though not more than four inches long, and hanging three inches apart.

1312. By simply bringing the gold leaves nearer to each other I was able to show the difference of specific inductive capacity when only thin plates of shell lac were used, the rest of the dielectric space being filled with air. By bringing B and C nearer to A, another great increase of sensibility was made. By enlarging the size of the plates still further power was gained. By diminishing the extent of the wires, &c., connected with the gold leaves, another improvement resulted. So that in fact the gold leaves became, in this manner, as delicate a test of *specific inductive action* as they are, in Bennet's and Singer's electrometers, of ordinary electrical charge.

1313. It is evident that by making the three plates the sides of cells, with proper precautions as regards insulation, &c., this apparatus may be used in the examination of gases, with far more effect than the former apparatus (1187. 1290.), and may, perhaps, bring out differences which have as yet escaped me (1292. 1293.).

1314. It is also evident that two metal plates are quite sufficient to form the instrument; the state of the single inductive plate when the dielectric is changed, being examined either by bringing a body excited in a known manner towards its gold leaves, or, what I think will be better, employing a carrier ball in place of the leaf and examining that ball by the Coulomb electrometer (1180.). The inductive and inductive surfaces may even be balls; the latter.

being itself the carrier ball of the Coulomb electrometer (1181. 1229.).

1315. To increase the effect, a small condenser may be used with great advantage. Thus if, when two inductive plates are used, a little condenser were put in the place of the gold leaves, I have no doubt the three principal plates might be reduced to an inch or even half an inch in diameter. Even the gold leaves act on each other for the time as the plates of a condenser. If only two plates were used, by the proper application of the condenser, the same reduction might take place. This expectation is fully justified by an effect already observed and described (1229.).

Observations on Dr. Faraday's Eleventh Series.

Our readers are already aware that in former parts of these "Annals" we have had occasion to notice considerable errors which are observable in various series of Dr. Faraday's *Experimental Researches in Electricity*: and we are happy to learn that our remarks have been productive of considerable experimental enquiry (even amongst some of our author's particular friends), which is one of the great objects for which this periodical was first established: and certainly the only method of arriving at those important truths on which this branch of scientific knowledge must ultimately rest.

The grand theme of the Eleventh Series is *Induction*, which Dr. Faraday has attempted to investigate in a variety of capacities: but with what success these investigations have been attended our readers will necessarily form their own opinions; our business being merely that of pointing out a few particulars essentially connected with the subject: and showing their bearing on one another, and on the theories they are intended to support.

Respecting electro-decompositions, Dr. Faraday supposes that the "*first* step is an electro-polarization of the compound particles operated on, and *decomposition* the second" step in the process (1164). To this doctrine we can have no objections whatever, because we have entertained the same idea ourselves several years ago, as may be understood by consulting page 19, Vol. I, of these Annals: nor could we have any objections to the idea of polarizing contiguous particles of atmospheric air or of specific induction, provided we were favoured with any experiments in support of it. Dr. Faraday's cage experiments (1173) have thrown no light on either of these points, nor have they proved any thing not previously known.

The experiment which Dr. Faraday made with the electrometer in the cage (1174), was obviously a repetition of one of those very interesting experiments which Beccaria made with his "*electrical well*"* more than seventy years ago: the "well" in one case being, *virtually*, the cage in the other: and the "delicate gold leaf electrometer" of Dr. Faraday, the *scrutator* of the illustrious Italian. As far as Dr. Faraday pursued Beccaria's experiments, the results were the same; but when, "by working the machine, the air within this chamber could be brought into what is considered a highly electrified state;" and that, "every attempt to charge air bodily. . . failed (1173)," Dr. Faraday's experiments became truly original, and the results of a very different character to any thing ever obtained by Beccaria. Nor do we remember that our Italian philosopher has any where inferred that, "if any portion of the air was electrified, as glass or other insulators may be charged, it was accompanied by a corresponding *opposite* action *within* the cube (1173):" from experiments in which, "neither during the charge or after the discharge did the electrometer or air within (the cube) show the least sign of electricity (1174)."

Dr. Faraday *living in the cage* was the very worst situation he could possibly have chosen to *live in* whilst attempting to ascertain the electrical condition of its contents: for being surrounded by equal electrical forces he had no means of discovering any of them. An electrometer placed in the centre of force of an highly electrized cloud, would not indicate electric action, because of the surrounding forces balancing one another. If Dr. Faraday, whilst in the cage, had imitated the dwarf in the lantern, by holding his electrometer out at the door, in the manner which the little man holds out his bell, he would have found very different results to those he obtained *within*.

We do not mean to follow Dr. Faraday through all his evolutions of *induction*, as our principal object is to place those *experiments* on which the theory depends in a proper light before our readers. The globe experiments commencing at paragraph 1208, are exceedingly ingenious; and if they could be repeated, in other hands, with the same exactitude of results as represented by Dr. Faraday's figures, they would certainly be of great interest. In the first experiment we find the original charge of app. 1 to be 254° : but on a second trial it was reduced to 250° : and that the sum total of the divisions of the charge was $122+124+2+1=249^{\circ}$: or just

* Beccaria's Artificial Electricity. London, Quarto Edition, p. 188.; also Cavallo's Electricity, Second Editon, p. 196.

one degree less than the whole charge before division. Now we are told in paragraph 1250, that six minutes are necessary to pass through the manipulation, after the first charge is taken: "three minutes pass between the first charge of app. 1 and the division; and three minutes between the division and the discharge, when the force of the non-transferable electricity is measured." Now it cannot be doubted that the same space of time would be required to ascertain the *latter* state of the whole charge as would be required for the *first*. But during the time the *first* state was measured, there occurred a loss of 4° , and, consequently, a considerable loss would occur during the time occupied in measuring the *latter* state of the whole charge in app. 1 before the division was made. The words "divided and instantly taken" do not imply that the division was made at the instant the carrier left app. 1, for to ascertain the *latter* state of the whole charge at the electrometer. The division took place after the carrier had returned a second time from the electrometer; which must necessarily have been the case; for we find (1207), that the carrier was back again "touching one of the balls" of the apparatus at the time the division was made. Under these circumstances then, if 4° were lost during the *first* measurement, of the whole charge of app. 1, it is fair to infer that 3° , at least, would be lost during the second measurement. Therefore the sum total of the *divisions* of the charge as stated (1208), would be 2° greater than the whole charge itself. Moreover, as three minutes elapsed in measuring the divisions, there would still be another loss to be brought into account: and as there would also remain in the two globes residual charges which the carrier could not take away, the sum of the *parts* of the charge would obviously appear considerably greater than the original charge itself; showing pretty clearly that either the apparatus were very unfit for the enquiry, or that the experiments were very inaccurately performed. Dr. Faraday, however, has squared his figures pretty well to make "both ends meet," without noticing these losses and residuals of charge which ought to have been accounted for.


Having given one specimen of the globe experiments we must leave our readers to judge for themselves of the rest of them. We shall not proceed with our observations on "induction in curved lines (1215)" at present; but shall place before our readers the *origin* of the experiments and the explanations of the phenomena given by their authors, in our next number.

EDIT.

XXIX. *On a phenomenon observed while experimenting with a Wollaston's battery of a large size.* By E. LENZ.

(Communicated by the author to the Editor of Poggendorff's Annalen, and translated from vol. 47, p. 461 of that work.)

It is well known that, in accordance with Ampère's theory, two succeeding particles of one and the same galvanic current mutually repel each other, and that Ampère bore out this deduction from his theoretical views by an experiment we find cited in all our manuals of electro-magnetism, namely, that a duly bent conducting wire resting on two disconnected surfaces of quicksilver acquires a sailing motion the instant these surfaces are brought into connexion with the polar wires of a galvanic battery.

Some time ago when at Professor Jacobi's I witnessed a phenomenon evidently identical with that I have just mentioned, but of a remarkable degree of intensity. The battery consisted of twelve pairs arranged as has been stated, and each presenting (taking only one side into account) a surface of three square feet of zinc. The plates were fastened to a frame; and the troughs, which were all filled with a very active mixture of diluted sulphuric and nitric acids, were so placed on a board that by means of a winch and wheel-work they could be raised up to them. The separate plates were so connected as to form a continuous whole by means of thick copper wires of this shape , their legs dipping down into mercury cups that were screwed on to the zinc and copper plates.

The action of this battery was so energetic that it at first raised to a white heat, and then melted through in the middle, a platina wire 0,125 inch thick, and $3\frac{1}{2}$ inches long; that is to say, the length of the connecting copper wires.

The most remarkable phenomena that we observed with this battery was however the following, namely, that on completing the circuit in itself by means of the copper connecting wires only and then raising up the troughs, as soon as the greater part of the surface of the plates became immersed **THE WHOLE OF THE CONNECTING WIRES SPRUNG OUT OF THE MERCURY CUPS WITH A LOUD CRACKLING NOISE.** We can, it is clear, only explain how the wires are thus thrown out by means of the repulsion we have alluded to, and which is here called forth at the places where the particles of the current in immediate succession pass out of the mercury into the connecting wire, and from this again into the mercury of the next pair of plates. It is however remarkable that this power of repulsion should be strong enough to heave up wires, each of which weighs 210 grains.

The like was also observed when the battery did not consist of the whole of the twelve pairs in connexion, but only of some of them ; this, however, is in strict accordance with Ohm's law, according to which the strength of a current is the same, be the number of the elements composing the battery what it may, provided it is not interrupted by the insertion of any foreign conductor.

JULIAN GUGGSWORTH.

Wormwood Scrubbs,
September 11, 1839.

XXX. *Supplementary Note to Article XVI. p. 190.*

The lightning which fell on the *Royal Hotel of Invalides*, on the 8th of June last (see p. 215), has produced effects from lateral explosions much greater than any I ever heard of before, and highly corroborative of the inferences I have drawn respecting the "probable effects" which similar explosions would have on Mr. Harris's conductors (206). The projecting of the nails from the lead on the lantern, the lifting up of that great mass of lead, and the entire removal of "the lead surrounding the base of one of the columns of the lantern", are ample proofs of the astonishing effects of lateral explosions : and certainly the best data which could be furnished for supposing that Mr. Harris's conductors are liable to be peeled from the masts by a similar kind of action.

I have already mentioned that lateral discharges were extensively studied by Viscount Mahon ; but, perhaps, the experiments of Professor Henry, of New Jersey College, in the United States, would be more to my purpose. This philosopher informed the British Association at the Liverpool meeting, that he had taken sparks from various parts of a lightning rod erected upon the best principle, and well connected with the ground, when that rod was carrying a discharge of electricity ; and stated that he believed that no rod could be sufficiently uninsulated as not to produce lateral discharges.

Since my fourth memoir was printed I have been favoured with the opinions of several naval officers respecting my plan of protecting shipping from lightning ; and having availed myself of some valuable suggestions as to the probability of those branch conductors which I had proposed to be placed before the shrouds of each mast, being liable to injury by pressure from the yards when the ship is close hauled, I have dispensed with them altogether. The system will thus be much simplified, and give an opportunity of placing the whole of the metal in those conductors which are *aft* the shrouds,

and entirely away from the masts. This plan will also remove every apprehension which might have been entertained of the shrouds being ignited in consequence of lightning heating those conductors which were in partial contact with them.

Fig. 8, Plate IV. represents a vertical section of the *starboard* and *larboard* conductors of each mast from the topgallant mast head to the copper sheathing of the vessel; and the darts show the manner in which a flash of lightning would be conducted down both sides of the rigging to the sea. Should a flash of lightning happen to strike either of the topgallant mast branches, then, because of their metallic union above, at the mast head, the lightning would be distributed through both branches, as decidedly as if it had struck the spindle above the truck. The topmast conductors being also metallically connected at the crosstrees would each conduct its own share of any flash of lightning which should happen to strike on either the *starboard* or the *larboard* branch. The lower mast conductors would also reciprocally and mutually assist in conducting lightning which should strike either of them. Hence, therefore, on whatever part of a conductor lightning should strike, it would invariably find two conducting channels to the sea, one on each side of the ship.

Fig. 9, gives a side view of the *starboard* and *forestay* conductors of a three masted vessel; also a view of the main topmast and mizen topmast stay conductors, which might be applied if thought necessary. These latter would be the means of uniting all the conductors into one system, from the topmasts' heads downwards to the sheathing. By this means every flash of lightning striking any branch conductor of the system would find its way to the sea through seven conducting channels, three on each side of the ship and one from the fore stay conductor which terminates under the bows. The small darts in fig. 9 show the distribution of the electric fluid from a flash of lightning supposed to strike the main-topgallant mast head, or the vane spindle above it. Lightning thus extensively distributed amongst distant branches of the conducting system would be rendered perfectly harmless.

The fastenings once completed, the conductors could at any time be put up in a few hours, and remain stationary during a cruise or a voyage, as decidedly as the standing rigging.

Mr. Harris's answer to my letter of September 12, is dated "Plymouth, September 15th," in which he informs me that he was Secretary of the Physical Section of the British Association, at Birmingham, but did not see or hear of my papers.

W. S.

XXXI. *Reviews and Notices of New Books.*

The Universal Ready Reckoner, or Royal Road to Arithmetic; being instructions in the use of the Sliding-Rule: compiled for the use of Ladies and idle Gentlemen. By an IDLE GENTLEMAN. JOHN LEE, 440, West Strand, near the Lowther Arcade.

The author of this little work is, in our estimation, so far from deserving the title which he has given himself, that we consider him a very industrious and useful man: and we heartily recommend the *Royal Road to Arithmetic* to that class of gentlemen to whom it is addressed; not for the purpose of indulging in their idleness, but as an example to induce them to bring their talent into active and useful play, and devote their time to those mental pursuits which are the strong bulwarks against the invasions of lethargy and vice, and the sure means of becoming benefactors to mankind, and of attaining true honour and lasting reputation.

"The Rule is justly described as most useful to those engaged in long calculations, however skilful they may be in arithmetic, both as saving much time in many cases, and more especially in acting as a check to detect and remedy errors which are always liable to creep into the calculations of the most skilful, and which if undetected, will vitiate a whole process.

"Its great use, however, is to the unskilful, and especially in the facility it gives in multiplying and dividing fractional and mixed numbers, in measuring the surfaces and solidity of various things: in short, its acting as a complete universal Ready-reckoner, with little more trouble than is necessary to understand a printed ready reckoner for one thing only. Moreover, nobody can make themselves tolerably *au fait* at the Rule without at the same time improving themselves, if ignorant or deficient, very much in the science of arithmetic. The time and trouble it saves and the accuracy it gives, and the power of doing things which would have been deemed by most people utterly hopeless, are beyond all praise.

"I trust, therefore, that old as the rule may be, considering how little it is known in the fashionable world, I am not over presumptuous in hoping, I may have done some service in calling afresh the attention of individuals to it. The first part of the book is addressed to the most ignorant and most idle, whom, by restricting myself to the very simplest directions, I have endeavoured not to disgust or confuse. In the second I have entered more into detail, and given such reasons and explanations as may satisfy laudable curiosity and give some useful instruction apart from the actual use of the instrument."

We have perused this little volume with great pleasure, and consider it our duty to recommend it to every class of professional men, and artificers, who are desirous of availing themselves of the assistance of the sliding Rule.

XXXII. MISCELLANEOUS ARTICLES.

Mr. Leeson, who has succeeded Mr. Phillips to the lectureship at St. Thomas's Hospital, has lately been engaged in a series of experiments for the purpose of ascertaining the amount of pressure which would stop the electro-chemical decomposition of water by voltaic electricity of a certain tension; and has discovered that about 50 atmospheres is capable of preventing decomposition from an electric force of a voltaic series of about 20 pairs. Mr. Leeson has employed cylinders of glass about 3 inches long and nearly 4 inches diameter, with a hollow axis the diameter of which is about $\frac{3}{4}$ of an inch, leaving the solid glass of more than an inch and a-half thick. The ends of the cylinder are ground perfectly flat and smooth, and are covered with flat iron plates of considerable thickness, also ground flat and smooth, in order to form air-tight joints with the two ends of the glass cylinder. The cylinder, being placed on one of the iron plates, is filled with water, and the other plate is then placed on the other end; and both plates kept firmly in their places by bolts and screw nuts which hold all together. It is to be understood that there is a contrivance for fixing two strips of platinum within the cavity of the cylinder, and for connecting them with the poles of the battery employed. Having thus described the mode in which Mr. Leeson proceeds with his interesting experiments, it remains that we inform our readers, that we have seen the fragments of one of those glass cylinders which was split by the internal pressure of the liberated gases from the decomposition of water. The broken cylinder which we saw in Mr. Leeson's laboratory, was at least an inch and a-half thick of solid glass; which seems to have broken by a pressure of a little more than 50 atmospheres. EDIT.

On a remarkable property of Electric Tension.

M. C. Doppler, Professor of Mathematics, at Prague, has said that he has discovered that a feeble electric charge given to a bar of insulated metal causes it to contract; and, of course, if negatively electrized, the metal expands. The following is the method employed in the experiment:—

A thin copper tube, about three feet long, is properly placed in a delicate compound lever-apparatus, something similar to our lever-pyrometers, and properly insulated by glass stems. If now, a feeble electric tension be given to the tube, the index of the lever-apparatus immediately begins to move, indi-

cating a *contraction* of the tube. By increasing the *tension* the motion of the index is so swift that the eye cannot trace it ; so that for high electric tensions a simple lever-apparatus answers better than the delicate compound one. When sparks are either intentionally taken from the tube or that they are discharged spontaneously, the index rapidly recoils, indicating an *expansion* (prolongation) of the tube ; which *contracts* again by an increase of electric tension. This property of electricity is said to have been discovered by a single pair of plates ; and, as Poggendorff has justly said, "probably by an electric current," which would render the circumstance still more curious, unless the current produced a depression of temperature in the tube. *From Poggendorff's Annalen der Physik und Chemie.* EDIT.

Mr. Mason, of High Holborn, who is an exceedingly dexterous experimenter, has lately discovered a method of enhancing the beauty and splendour of charcoal deflagrations by voltaic electricity. He powders bichromate of potash and places the powder on the points of the charcoal, and when the latter has become red hot, the bichromate also enters into deflagration. The light produced is two-fold that given by the charcoal alone. The potash becomes decomposed and the liberated potassium scintillates in a very beautiful manner. We have seen Mr. Mason produce a beautiful effect with a battery of only thirty small jars. EDIT.

Speaking of deflagrations, perhaps it may be interesting to some of our readers to know that the laminated metals, such as gold, silver, or copper leaf, &c., give a good effect when deflagrated on a strip of bright tin plate, which may be connected with one pole of the battery, whilst the metallic leaf, placed on a long copper wire in connexion with the other pole, is passed gently over its surface. Gold gives the feeblest light, copper next. The light from white Dutch metal is much more brilliant, and of a pale purple tinge. That from silver is exceedingly handsome, being a pale greenish yellow. We have, for many years, used zinc turnings for deflagrating at the lecture table. They burst into a complete blaze, and become converted into *philosophical wool*, which floats in the air for a long time afterwards. EDIT.

My Dear Sir,

I have received your very interesting paper on successions, or series of electric currents, with which I am quite delighted. You will perceive, by looking at page 112 of vol. 2 of the *Annals*, that I had proceeded as far as tertiary currents, by means of the Magnetic Electrical Machine; but you have gone much farther, and the results of your experiments are really beautiful. I shall place them before our English experimenters in the January number of the *Annals*.

My principal object for making this letter public is, that of giving philosophers an opportunity of testing my theory of magnetic electricity, as given in vol. 1. of the "*Annals of Electricity, &c.*" with your extensive series of successive currents. You have given a series of symbols expressive of the direction of the currents to the fifth degree, by *opening the circuit*. Now, if my theory be correct, I can predict the direction of the currents which would be brought into play by *closing the circuit*, which series will be very different to that given by *opening the circuit*. The two will stand as follows:—

On opening the Circuit.		On closing the Circuit.	
Primary Current +		+
Secondary Current +		—
Current of the 3d order	—		+
Ditto, 4th order	+		—
Ditto 5th order	—		+
	&c.		&c.

I have not tried the experiment, but I have no doubt of your finding the currents as I have predicted from an application of the principles of the theory.

I am, my dear Sir,

Yours very truly,

W. STURGEON.

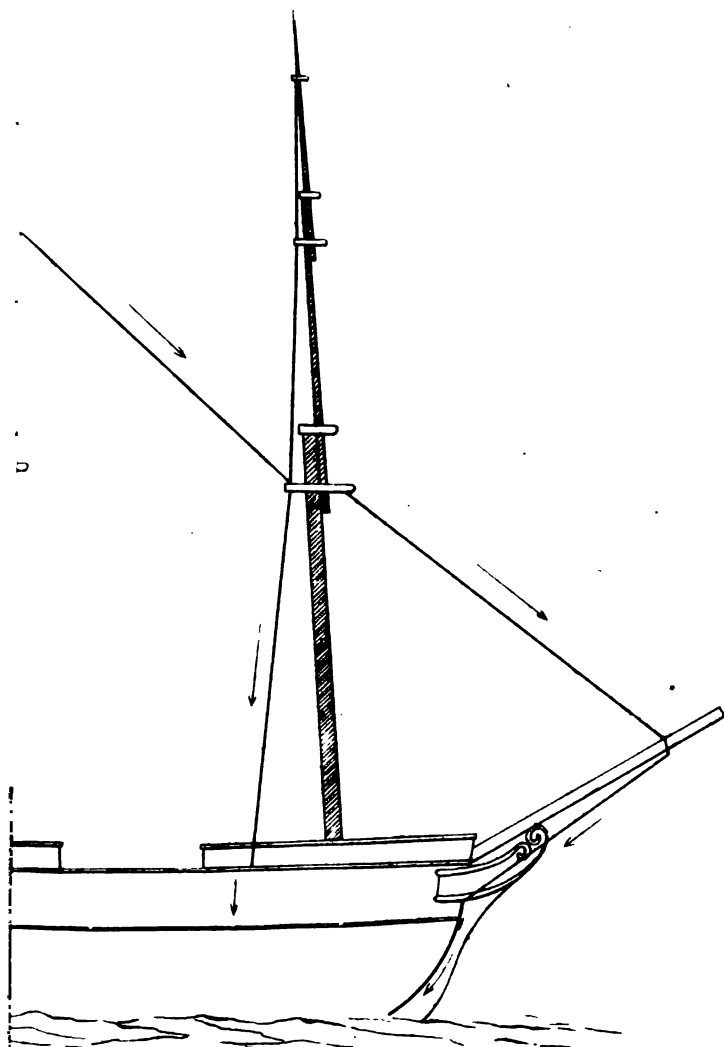
JOSEPH HENRY, Esq. L.L.D.

Prof. of Nat. Philosophy,
In the College of New Jersey,
Princeton.

NING CONDUCTORS as applied to a
el, by

RGEON.

*Main-top gallant Mast would find its way
tion indicated by the small arrows.*



ward and the stay conductors.

Fig 3

Fig. 1.

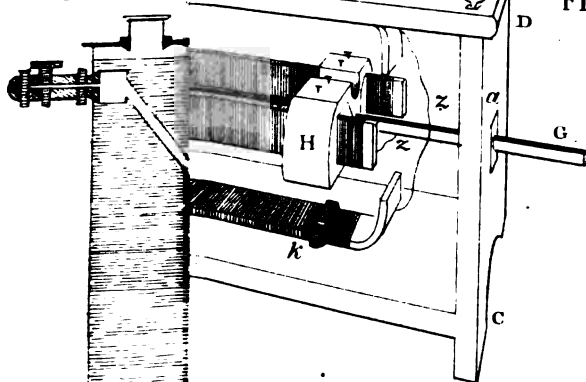


Fig. 6.

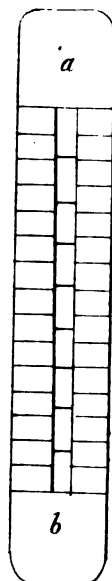


Fig. 9.

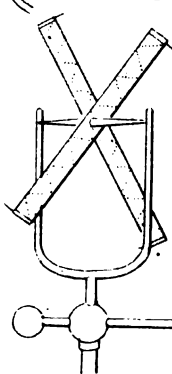
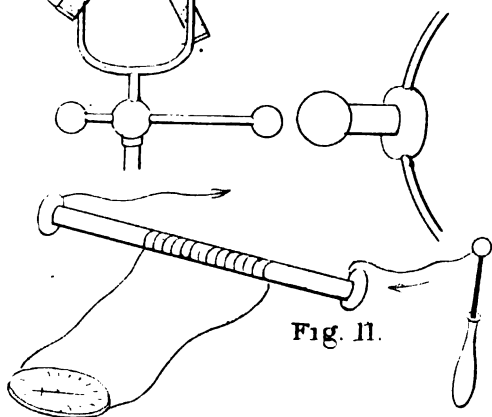


Fig. 11.



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Twelve-inch plate Electrical, 5*l.* 5*s.*

Twelve-inch plate Electrical Machine, with apparatus necessary for medical purposes, packed in a case, complete, 7*l.* 7*s.*

Fifteen-inch plate Electrical Machine, complete, 9*l.* 9*s.*

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Plate Electrical Machines of all dimensions, constructed on the plan proposed and recommended by Mr. W. S. Harris. They are of a very convenient and original form, act very powerfully, and possess positive and negative conductors.

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- See-saw Apparatus. This Toy illustrates in an entertaining manner electrical attraction and repulsion, 15s.
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- Electrical Swan, 2s.
- Electrical Sportsman, consisting of a carved figure represented in the act of shooting, and a Leyden jar having two wires of different lengths affixed in its cap. On the end of the longer wire small pith birds are suspended by threads, 1l. 5s. to 1l. 16s.
- Glass Globe, for experiments on electrical light in a medium of variable or invariable density, 1l. 5s. to 1l. 11s. 6d.
- Luminous Conductor, for showing the vivid effects of flashes of electrical light, which are more or less in perfect continuity and regularity in proportion to the degree of exhaustion within the tube, 18s. to 2l. 2s.
- Glass Flask, also for showing the passage of an electric discharge from one conductor to another, 7s. 6d.
- Apparatus to exhibit the effect of a falling star, 18s. to 1l. 5s.
- Luminous Conductor, with iron chain inside a bent glass tube, 10s. 6d. to 15s.
- Glass Tubes, with small variegated circular disks of tinfoil pasted round them in a spiral form, 5s. 6d. to 12s.
- Five Glass Tubes, with tinfoil spangles, mounted on mahogany frame, with an insulated revolving brass needle having free motion on its point of suspension, 1l. 8s.
- Glass Tube, with tinfoil spangles, mounted on mahogany foot. On the top of the tube are placed two cross wires, forming a fly or whirl, 13s.
- Glass Plane, painted in different transparent colours, with devices formed of tinfoil spangles, 13s.
- Names or Words arranged upon glass plates, with pieces of tinfoil, 10s. 6d.
- A Mahogany Stand for experimenting with electricity upon eggs, 7s. 6d.
- Bennet's Gold Leaf Electroscope, 1l. 3s.
- Bennet's Electroscope, with small condenser attached, 1l. 11s. 6d.
- Singer's Electroscope. This instrument in principle is the same as that suggested by Bennet; but the brass wire, supporting the gold leaves, is defended from the variation of the atmosphere by double insulation, 16s. to 1l. 6s.
- Singer's Condensing Electroscope. This instrument consists of Singer's electroscope with a condenser attached, 2l. 2s.
- Hare's single-leaf Electroscope, with horizontal sliding wire and ball passing through the side of the receiver, 18s. to 1l. 5s.
- Bohnenburger's single-leaf Electroscope, with two sliding balls and wires on opposite sides, with the ends of a small dry pile attached to them, 2l. 2s. to 3l. 3s.
- Cavallo's Electroscope, well adapted for experiments on atmospherical electricity, 1l. 1s.
- Haüy's Needle Electroscope; employed chiefly in ascertaining the electrical state of mineral substances, 8s.
- Harris's delicate Needle Electroscope. The needle is composed of a long gilded reed, terminating in a pith ball placed on a short curved arm of brass, and counterpoised by a shorter reed, and weight placed on the stem, 10s. 6d.
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- Cuthbertson's Compound Universal Discharging Electrometer.** The electric forces are estimated in grain weights, 2*l.* 5*s.* to 2*l.* 12*s.* 6*d.*
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- Lane's Discharging Electrometer.** Estimates the accumulated charge of an electrical jar, by measuring the distance in air which the discharge is capable of traversing, 7*s.* 6*d.*
- Harris's Improved Lane's Electrometer.** The exploding balls of this instrument are supported between a bent glass arm and a vertical tube of brass, and may be set at any given distance by means of a graduated slide, 2*l.* 2*s.*
- Harris's Electro-Thermometer.** The force of electricity measured by the relative heating effects producing upon two different metallic wires placed air-tight through the glass bulb, 1*l.* 11*s.* 6*d.*
- Harris's Hydrostatic Electrometer.** The object of this instrument is to investigate electrical action through the medium of the attractive and repellent forces operating between two plane conducting surfaces; the force being indicated on a graduated arc, and referrible, if required, to any given unit of weight, 10*l.* 10*s.*
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- Harris's Unit Jar Electrometer.** This instrument is used to measure the quantity of electricity actually conveyed into a battery or large jar, 1*l.* 1*s.*
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- Electrical Inclined Plane,** formed by two inclined wires stretched between four insulated pillars, and a fly or whirl is placed across the wires, 1*l.* 1*s.*
- Electrical Planetarium.** An experiment in which the dispersion of electricity from a point produces a rotatory motion by the reaction of the resisting medium of the air, 8*s.* to 1*l.* 4*s.*
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- Square Glass Plate** in mahogany frame, partially coated with tinfoil, to prove the insulating faculty of glass by its supposed impermeability to electricity, 15*s.* to 1*l.* 5*s.*
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- Medical Electrical Jars,** with contrivances to qualify the intensity of the charge, 7*s.* 6*d.* to 10*s.* 6*d.*
- Electrical Jar,** with moveable metallic coatings, to demonstrate that in the electrical jar the electricity is accumulated upon the glass and not on the coating substances, 12*s.* to 14*s.*
- Electrical Jars,** with spotted and diamond coatings. These jars form a brilliant and beautiful appearance in a darkened chamber, 6*s.*, 10*s.* 6*d.*, 18*s.*, 1*l.* 4*s.*
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- Electrical Batteries** of various sizes and numbers of jars. By means of arrangements of this kind, quantity and intensity of the electric fluid may be obtained to any extent, 15*s.* to 5*l.*
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- Mahogany Model, termed "Lightning House," which demonstrates the value of metallic conductors to houses in a thunder and lightning storm, 18s.
- Mahogany Model of an obelisk to illustrate the properties of lightning conductors, 11s.
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- Fire House to exhibit the intense heat evolved in the electric discharge, sufficient to ignite tow, saturated with resin, spirits of wine, or other combustibles, 16s.
- Lullin's Apparatus for exhibiting the perforation of compact bodies by the electrical discharge, 15s. to 1l. 5s.
- Two Balls of different diameters, with metallic coatings, for the purpose of illustrating the conditions of electricity usually termed *intensity* and *quantity*, 1l. 7s.
- Brass Rods, terminated by balls and points, for fitting into either the prime conductor of the machines, or fixing to insulating solid glass handles, 2s. 6d. to 7s. 6d.
- Balls of Brass, Ivory, Bone, and Baked Wood, of various diameters, for experimental purposes, 6d. to 3s. 6d.
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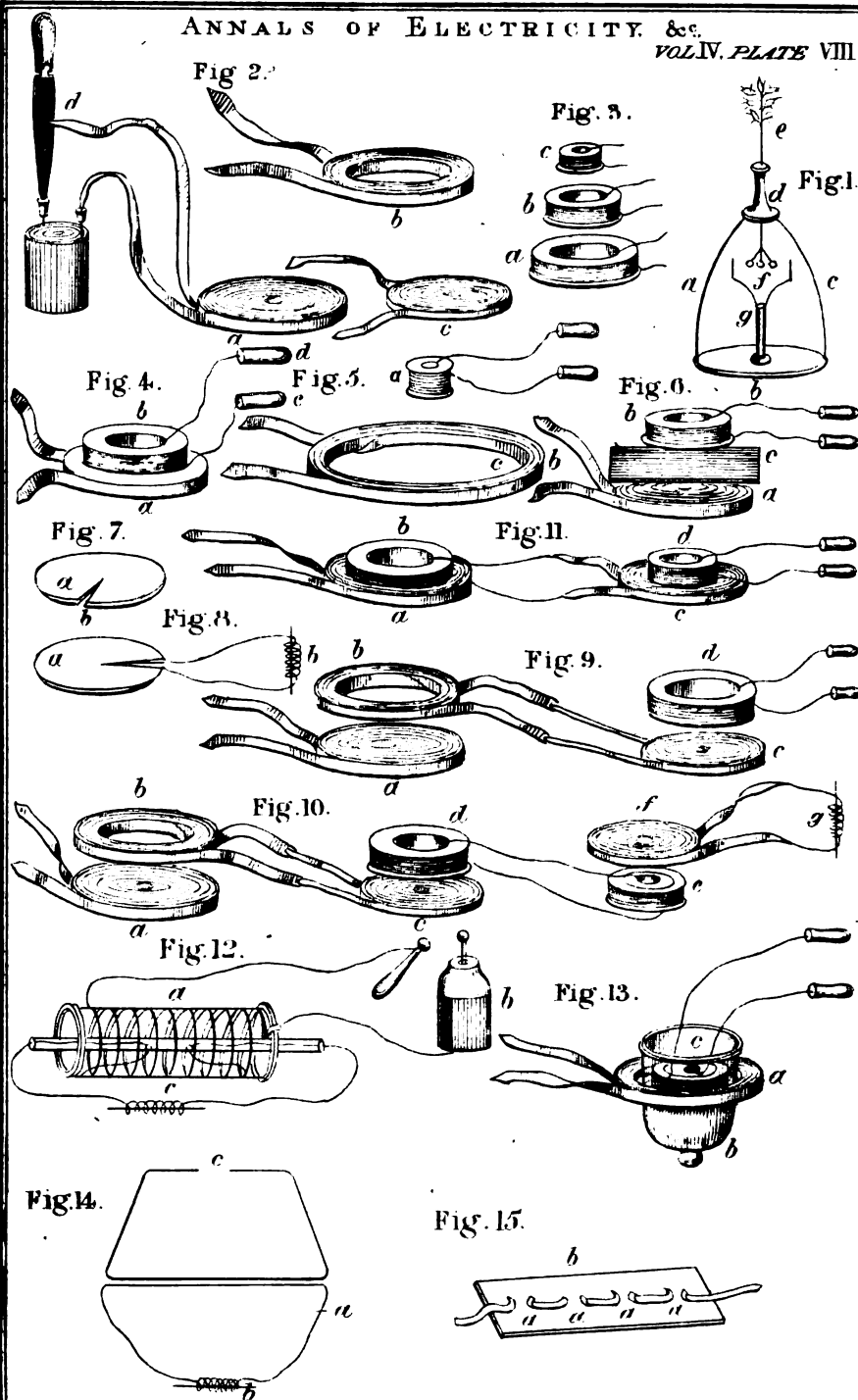


Fig. 1.

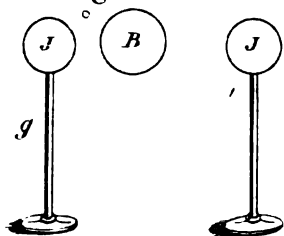


Fig. 2.

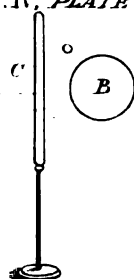


Fig. 3.

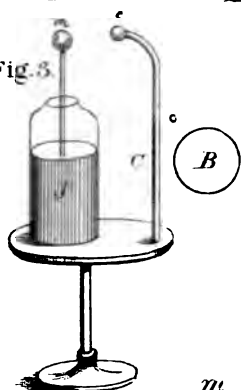


Fig. 4.

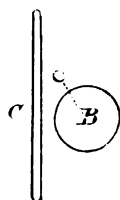


Fig. 6.

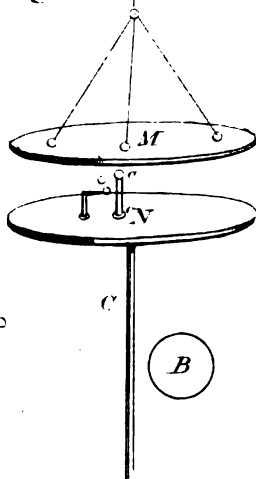


Fig. 5.

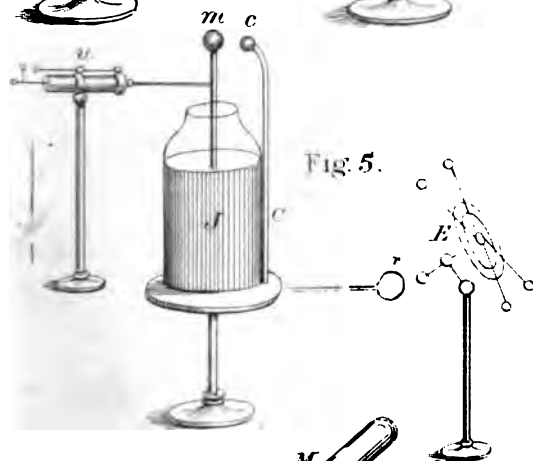
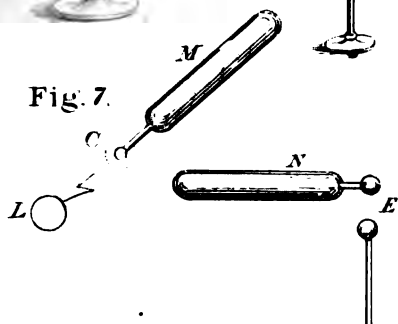


Fig. 7.



THE ANNALS
OF
*ELECTRICITY, MAGNETISM,
AND CHEMISTRY;*

AND
Guardian of Experimental Science.

JANUARY, 1840.

XXXIII. *On the connexion between Electricity and Vegetation.* By THOMAS PINE, Esq., Maidstone.*

Reflecting on the known properties of the electric fluid, and the suitability of plants in their relation to the surrounding elements to receive its influence, I was led to regard it as highly probable that vegetation depends much on this principle. From the experiments of Mr. Cavallo, as recently confirmed by those of Mr. Sturgeon,† I had learnt that the air is in a constant state of positive electricity; hence it seemed reasonable to conclude that the acute extremities of plants in a living growing state, must be constantly imbibing some portions of the fluid, and introducing it into their substance. I expected that their attractive energy must be considerable, as the mutual arrangement has much the appearance of an immense apparatus in constant operation; and was strongly confirmed in my conclusion by observing that a common blade of grass, when presented to the prime conductor of an electrical machine, gave evident proofs of a more potent attractive and conducting power than appeared in a corresponding metallic point; the fluid appearing to flow toward it with less obstruction, with a more uniform current, exhibiting a much brighter light, and at considerably greater distances. In some experiments made in the month of June, a metallic point and a vegetable point being held equidistant from the prime conductor, the vegetable point continued to

* Communicated by the Author.

† Mr. Sturgeon favoured me with a most obliging letter containing ample evidence from his numerous experiments both of the general fact and of many interesting particulars relating to the subject, the contents of which, I trust, will appear in confirmation of the above statement.

VOL. IV.—No. 22, January, 1840.

S

be illuminated till it had reached at least four times the distance at which the metallic point ceased to exhibit any light, that is at the distance of about fourteen feet. A corresponding effect attends the passing of the contents of a charged jar through a vegetable point; as in this case, the human body being a part of the circuit, the jar will be discharged with almost no perceptible effect on the animal frame, yet leaving hardly any residuum; whereas if a metallic point be employed, the shock will be more sensibly felt, and the residuum more considerable.

Hence it follows that vegetable points must be acting with a great and continued energy upon the electricity of the atmosphere, either in imparting to it the electric matter which it uniformly contains, or in imbibing the fluid from the atmosphere, which must be as constantly afforded to it from some other source. The latter conclusion appears by far the more probable; since it is impossible to account for continual supplies of electric fluid from an earth in a constant state of negation with respect to its atmosphere; whereas the atmosphere is constantly receiving solar rays which possess *some* if not *all* the properties of electric matter. There are so many points of resemblance, if not identity, between the phenomena respectively ascribed to light, caloric, and electricity, that much fewer difficulties will probably be found to attend the hypotheses that they are but different effects arising from one common cause or source, than from the conclusion that they are produced by so many distinct, yet all-pervading, fluids. If the ball of a thermometer be electrified, by first moistening it for an exterior coating, while the interior coating is formed by the mercury, a stream of light will shoot through the vacuum to the summit of the tube, showing that the fluid is essentially luminous. Is it not then essentially the same with light, and does not the latter possess electrical properties in common with the former? If this be admitted, it appears to me that vegetation, through every stage of its progress, from the germinating seed to the full grown and perfected plant, will be found admirably to accord with such influences from the sun, whether by his direct rays, or through the instrumentality of the air and vapours.

The several varieties of form and properties to which plants are subjected in their progress, seem adapted to corresponding electric influences from the respective elements.*

* I venture to use the old term, not having a better, to designate the principal divisions of our atmosphere, though two of them are now well known to be compound bodies.

Thus the seed in the loosened soil, the pointed germ issuing from its surface, and the bud on the branching tree, appear peculiarly fitted by their acute rigid extremities to receive the exciting influences of an electrified air, which, by its gradual swell and agitated movements, in the early spring season, presses against them continually in successive eddies, conveying to each of them some of its electric matter. It appears to me that this simple principle, in conjunction with an occasional supply of the same fluid from vapours, together with their moisture, will in a considerable degree account for the first excitement and germination of plants; especially when it is considered that every preparation seems made for adapting the atmosphere thus to act on the embryo plants at this season. Freed from vapours by the condensing effects of cold in the preceding winter, it is now in a peculiar state of dryness; and now the glancing rays of the sun accumulate in it and render it strongly electrical. This accumulation must be much favoured by the absence of foliage in the larger and more vigorous parts of the vegetable kingdom at this crisis; for the transpiration of moisture from the expanded leaf neutralizes myriads of solar rays, and charges the atmosphere with vapours; but, in consequence of the whole class of indigenous plants presenting nothing but minute buds from their ramifying branches, no rays are neutralized, and no vapours are formed, from them. Consequently all those rays, which would otherwise have been thus neutralized, are left floating electrically in the pure air, or entering its pores in small portions, serve a little to raise its temperature and swell its volume, and so to aid the general effect.

That the rays of the sun entering the atmosphere at this season, at acute angles with the earth, must tend considerably to cause them to lodge and accumulate in the strata above in the form of electricity seems evident in itself, and to receive confirmation from the very large accumulations of it in the polar regions at the periods when the sun is at his greatest distances from the zenith of the respective poles; for to what more probable cause can these agreeable and welcome lights be ascribed, but to a very large portion of the almost parallel rays resting in the higher and more attenuated regions of the atmosphere in those quarters; and thus in some degree administering the several benefits of light, warmth, and electricity, to those otherwise deserted parts of the globe? But we are not left to conjecture as to the fact, Mr. Sturgeon having generously informed me, as the result of his numerous atmospherical experiments, that the strongest electricity exists in the air at this season, and

under the influence of those cold drying winds by which it is distinguished: and this is in entire conformity with the observations I have occasionally made. The following experiment will tend to show its influence on vegetation.

On the 28th of April I sowed mustard seed in similar soils, contained in two jars, one electrified positively, the other negatively. The covers being removed they were both left open to the action of the atmosphere. In four days the plants appeared in both jars, but those in the negative jar were the most advanced; while no plants appeared till about two days later from a similar sowing at the same time, unelectrified. On the 12th of May the plants in the negative jar had grown to $2\frac{3}{4}$ inches, those in the positive jar to $2\frac{1}{4}$ inches, in height; those unelectrified rather remaining in the ordinary state to $1\frac{1}{2}$ inch. The electrified plants were vigorous and flourishing in proportion to their height. This result in favour of the plants negatively electrified must have arisen chiefly from the *relative* superiority of electric matter in the atmosphere; in the case of those positively electrified the *absolute* quantity was increased, but the relative difference between those and the atmosphere somewhat reversed. Hence it appears that, while much depends on electric influence, its *positive* state in the atmosphere as contrasted with the soil operates most effectually on vegetation. The difference of more than half in height in favour of the former, above those in the natural state, strongly encourages the conclusion that the constant accessions of electric fluid in the atmosphere at the spring of vegetation constitutes its influence, and that on the degrees of that influence depends in a great measure the rapidity and vigour of its rise and progress. A medical electrician, residing in this town, acquainted me with the following particulars:—A narcissus plant when in a very weak and languishing state being placed in the room in which his powerful machine was kept in frequent action, soon began to show signs of extraordinary vigour; it grew to the height of 36 inches, and was stout and luxuriant in proportion. Some branches of the moss rose, and various other flowers in the room, retained their colours while the seeds were forming, during about five weeks, and at length dropped off without losing their freshness. These and some other particulars which he related to me, on the correctness of which I have reason to rely, show the vast advantages that might be expected to result from a continued powerful electricity in the atmosphere, accompanied with a suitable temperature and dryness. If a plant can be made to expand to thrice its ordinary dimensions, by an artificial increase of positive elec-

tricity, and that in a confined situation, where the direct rays of the sun could hardly exert their ordinary influence, how important must be the operation of this principle, in proportion as it should seem, to the degrees in which it obtains! But it must be by the proper union of its several properties of light, heat, and electricity combined, that its greatest and most salutary effects on plants are produced; and accordingly by far the richest and most copious productions of the vegetable kingdom are found in those climes in which the solar beams are most abundantly distributed. A very satisfactory proof of the electric operation of those beams appears in the following extract from the Atlas, to which I was referred by my valued friend and coadjutor, Mr. Weekes. "For the double purpose of ascertaining the power of spines in modifying the electric relation of the atmosphere and the earth, and in effecting the progress of vegetation by their electric influence, M. Astier insulated a sextuple spine of the *gleditzia triacanthos* at the top of his house, and brought a wire to it from an insulated pot, in which were planted five grains of maize: a similar sowing was made in an uninsulated pot, for the purpose of comparison. The experiment continued from the 6th to the 20th of June, including two stormy days. The electrometer gave considerable signs of electricity in the flower pot, and, by using the condensor, sparks were produced. The electrified grains were found to pass more rapidly through the first stages of vegetation. When Bengal-rose trees were submitted to the same experiment, the flowers of the electrified plant appeared more rapidly and more abundantly than in the other case."

I trust it will have sufficiently appeared from the above statements that the commencing stages of vegetation are in a great degree caused or promoted by the influence of the electric fluid which is lodged in the dry air of our atmosphere at the season of germination. Additional evidence is, no doubt, highly desirable, and experiments of a more decisive and interesting character could easily be devised were the attention of those who have good opportunities of connecting the cultivation of plants with electrical inquiries more particularly directed to the subject. With respect to my leading position of the superior conducting efficacy of vegetable points, and their extensive influence on atmospheric electricity, the most accurate scientific proof will be seen in the annexed very obliging and admirable letter of Mr. Weekes. And I have only to add, on this head, that our correspondence took its rise from some hints he had received concerning my humble, but I trust not unimportant, discovery, and the inferences I

was beginning to deduce from it. The ardour with which he engaged in the inquiry will best appear from his own statements, from which I shall no longer suspend the attention of your readers; only congratulating them on his recent discovery of the decomposition of water by vegetation, as related by him in the last Number but one of these valuable Annals of Electricity; a discovery which must form a most interesting addition to general science, as well as much assist our researches into the principles of vegetation.

(To be continued.)

Sandwich, May 31, 1828.

Dear Sir,

Various circumstances have united to occasion a tedious delay in our correspondence since I last addressed you, and promised an investigation of your ingenious theory of electricity. However, I find I have by no means had too much time for a fair and impartial examination of the subject, the interest of which, to me, has been such as to excite experiment far beyond my intentions at the outset. The final result in my mind is an entire conviction that your opinions are well founded, and have stood the test of the severest trials to which they could be subjected. The vast superiority of vegetable over metallic points in the drawing off and accumulating electric matter, is, I conceive, a subject of great interest and importance. A coated jar having 46 inches of metallic surface was repeatedly discharged by the activity of a vegetable point in 4 min. 6 sec.; while the same jar charged to the same degree, required 11 min. 18 sec. to free it from its electric contents by means of a metallic point: the points in both cases being equidistant. I find also that Benett's gold leaf electroscope (a delicate instrument) is powerfully affected by a charged jar, at the distance of nearly 7 feet, when the brass cap of the instrument is furnished with a branch of the shrub called *butcher's broom*, and which I have found of great use in my experiments. The same delicate instrument when mounted with pointed metallic wires is not perceptibly affected until the charged jar approaches to within 2 feet of the cap. I must not think of troubling you with the details of all that I have been about as regards this investigation; but one circumstance has proved too pleasing to be wholly omitted.

Let *a, b, c*, fig. 1, Plate VI, represent a large street lamp in an inverted position, mounted with a brass cap *d*, through

which passes a stout wire *ef*, having a brass knob at *f*, and a pair of small pith balls attached to the wire just above the knob. *g* is a portable stand with two metallic discs, one on each side of the wire and rising to a level with the pith balls. *h* is a small branch of the butcher's broom fixed by a twine to the upper extremity of the wire *ef*. This apparatus I have for many weeks past had in almost daily use, nor can I express the pleasure it has afforded to myself and friends by its frequent indications of atmospheric electricity; for, armed with your *vegetable detectors*, it has shown symptoms of electricity by the passing of clouds at a great altitude, and under various other circumstances in which electrometers with metallic points placed by its side gave no indications whatever. This appears to me so decided a proof of the superiority of vegetable conductors, that it admits of no contradiction.

The correctness of your opinions respecting the influence of electricity upon the growth of plants, appears to me to be sufficiently proved by the following experiments. Two small flower pots filled with rich mould were taken for the purpose in doors. A few grains of mustard seed were sowed in each; both were kept gently watered, but one pot was *insulated* and frequently electrified under circumstances which kept it, as it were, in an electrical atmosphere. The other pot had no such attention shown to it, and the result proved what you probably would anticipate. The vegetation of the electrified seeds appeared several days before the others, and continued afterwards to grow with a much greater degree of vigour. As a lover of science you can easily imagine the pleasure these pursuits have yielded, and to this has succeeded an anxious desire that you should speedily assert your claims, or I apprehend you will lose your just title to originality. I send you an extract which lately fell in my way; the perusal of which I hope will put you on the alert. The extract is from Taylor's "System of Philosophy," in which the author has written a great deal of downright nonsense; but still it appears he had somewhere obtained a *glimpse* of the same opinion by which you are animated on this subject.

"The leaves of plants act as so many spicula to attract the electricity of the air and *solar rays*; hence very high trees are so many natural conductors, attracting a vast quantity of electric fluid, and, consequently, put forth a luxuriance of foliage proportionate thereto." *Review of Books. Quarterly Journal of Science, October, 1826.*

You see by these approaches towards your theory and facts, you ought to lose no time in securing the just praise of origin-

ality, to which you are doubtless entitled. My health being now perfectly restored I am very actively engaged, or I could willingly have written at greater length, but must conclude with saying,

I am, dear sir,

Very faithfully yours,

W. H. WEEKES.

To Thomas Pine, Esq.

Woolwich, Dec. 2d, 1832,

My dear Sir,

It is with a very great deal of pleasure that I have read your letter stating your intention of publishing your views on some of the most interesting phenomena of nature, and permit me to acknowledge that I feel much honour by your selecting my humble authority in giving assistance to your efforts; and I can assure you that nothing shall be wanting on my part, as far as experience has enabled me to draw conclusions, to forward your very laudable object.

In the first place, then, I perfectly agree with you, as to the solution of the results of Sir H. Davy's experiments on corn; for the positive pole of a voltaic battery would supply the animating electric fluid to the germinating seed in precisely the same manner that nature supplies it from the atmosphere to the ground. As Sir Humphrey does not state from what "experiments made on the atmosphere" he draws his conclusions "that clouds are usually negative," I am unable to form any opinion respecting them. But I must beg permission to state, that such a conclusion is quite at variance with the results of my experiments. It is true I have obtained negative charges at the kite string, but the instances are very few indeed. Those which did occur were only whilst heavy clouds passed over the kite; the indications, both before and after the clouds' transit, being invariably positive. And even in those temporary exhibitions of negative electricity, I am very far from concluding that the clouds themselves were negatively electric. The indications were those of the kite, which was floating much lower than the clouds: and the air *vicinal* to the kite was consequently the *only part* of the atmosphere explored during each experiment, which air probably became negative or deprived of most of its natural electricity by the repulsive force of the accumulated electric matter in the positively charged clouds. This assertion can hardly be construed into "begging the question" or "strain-

ing a point;" because such phenomena are easily produced by experiment and must necessarily frequently occur in nature.

To ascertain directly, the electric state of clouds, the kite ought to be immersed immediately in those we wish to explore. No such experiments have yet been made. I am of opinion that my kites have been nearer to the clouds than any hitherto employed for experiments of this kind. Many experimenters have contented themselves with 400 or 500 yards of string, and others have drawn their conclusions without employing any kite in their experiments, from experiments made with an apparatus not much longer nor very unlike an Angler's Rod!!! The results which I have obtained from about 300 experiments, at nearly all seasons of the year, and at all times of the day, and many at night, induce me to believe that the *general* electric state of the atmosphere, with its contained clouds, vapours, &c., is, with reference to the earth, *positive*. For, notwithstanding those very rare aberrations from the general results which I have noticed at the kite string during the transit of a fleeting cloud, they appear to me (in the way which I think they operate) to be *favourable* than otherwise to the conclusions at which I have arrived. Moreover, I find from experience, that bodies generally, when in, what is usually called, their natural electric state, have *not* an equable distribution of the electric matter on every part of their surfaces; but, on the contrary, that each individual body or substance, when in this, its *natural state*, exhibits different electric tensions on various parts of its surface. So it is in the atmosphere, that at different times, and at different altitudes at the same time, different electric tensions are exhibited.

All electrical phenomena are *relative*, and consequently all our calculations respecting them, have no other basis but the ever varying degrees of those relations. But, notwithstanding the variations in the extent or degrees of those relations, the *relations themselves* appear to be constant and uniform. Therefore I conclude generally (and my conclusions are from direct experiments) that the atmosphere, taken as a whole, is constantly in an electro-positive state with reference to the earth; and that in the atmosphere itself, the upper regions are constantly electro-positive with reference to all those situated nearer to the surface of the earth. The strata of air near to the earth's surface are therefore in an *intermediate* state of electricity with reference to the upper strata and the body of the earth, the earth itself being negative to the whole.

These results, my dear sir, are, in my opinion, of a very decisive character; and if you deem them of sufficient importance to be taken into consideration whilst framing your

theory of electro-vegetation (pardon me for inverting the term "vegetable electricity") they are quite at your service; and if you are desirous of stating the authority, you are perfectly at liberty to do so.

Whilst writing this letter (between six and seven in the evening) a tremendous thunderstorm passed over this place. Half an hour before, the sky was quite clear, the moon and stars shone with great lustre.

I am, Dear Sir,

With very great respect,

Yours very truly,

W. STURGEON.

To Thomas Pine, Esq.

XXXIV. *On the Colours of Mixed Plates.* By SIR DAVID BREWSTER, K.G.H. F.R.S.*

Received October 25,—Read December 14, 1837.

The colours of mixed plates were discovered by Dr. Thomas Young,† and described in the Philosophical Transactions for 1802. He produced them by interposing small portions of water, or butter, or tallow between two plates of glass, or two object glasses pressed together so as to give the ordinary colours of thin plates. In this way portions or cavities of air were surrounded with water, butter, or tallow; and on looking through this combination of media he saw fringes or rings of colour six times larger than those of thin plates that would have been produced had air alone been interposed between the glasses. These fringes or rings of colour were seen by the direct light of a candle, and began from a white centre like those produced by transmission; but on the dark space next the edge of the plate, Dr. Young observed another set of fringes or rings, complementary to the first, and beginning from a black centre like those produced by reflection. This last set of colours was always brighter than the first.

The following is Dr. Young's explanation of these two series of colours.

"In order to understand," says he, "this circumstance, we must consider that where a dark object is placed behind the glasses, the whole of the light which comes to the eye is

* From the Transactions of the Royal Society for 1838.

† Since this paper was written I find that this class of colours was discovered by M. Mazeas, and that his experiments were repeated and varied by M. Dutour.

either refracted through the edges of the drops, or reflected from the internal surface ; while the light which passes through those parts which are on the side opposite to the dark object consists of rays refracted as before through the edges, or simply passing through the fluid. The respective combinations of these portions of light exhibit a series of colours of different orders, since the internal reflection modifies the interference of the rays on the dark side of the object, in the same manner as in the common colours of thin plates seen by reflection. When no dark object is near, both these series of colours are produced at once ; and since they are always of an opposite nature at any given thickness of a plate, they neutralize each other and constitute white light.”*

In so far as I know, these observations have not been repeated by any other philosopher ; and subsequent authors have only copied Dr. Young’s description of the phenomena and acquiesced in his explanation of them. In taking up this subject I never doubted the accuracy or the generality of the results obtained by so distinguished a philosopher. I was induced to study the phenomena of mixed plates as auxiliary to a more general inquiry ; and having observed new phenomena of colour in mineral bodies, which have the same origin as those of mixed plates, and which lead to conclusions different from those of Dr. Young, I am anxious that they should be described in the same work which contains his original observations.

Having experienced considerable difficulty in obtaining satisfactory specimens of the colours of mixed plates by using the substances employed by Dr. Young, I sought for a method of producing them which should be at once easy and infallible in its effects. With this view I tried transparent soap, and whipped cream, which gave tolerably good results : but I obtained the best effect by using the white of an egg beat up into froth. To obtain a proper film of this substance I place a small quantity between the two glasses, and having pressed it out into a film I separate the glasses, and by holding them near the fire I drive off a little of the superfluous moisture. The two glasses are again placed in contact, and when pressed together so as to produce the coloured fringes or rings, they are then kept in their place either by screws or by wax, and may be preserved for any length of time.

* Philosophical Transactions, 1802. Dr. Young republished the same explanation of mixed plates in 1807 in his *Elements of Natural Philosophy*. See vol. i. p. 470, 787 ; vol. ii. 635, 680.

If we now examine with a magnifier of small power the thin film of albumen, we shall find that it contains thousands of cavities exactly resembling the strata of cavities which I have described as occurring in topaz, quartz, sulphate of lime and other minerals;* and if we look through the film at the margin of the flame of a candle, we shall perceive the two sets of colours described by Dr. Young, the one upon the luminous edge of the flame, and the other on the dark space contiguous to it. The first we shall call the *direct*, and the second, which are always the brightest, the *complementary fringes*.

If we apply a higher magnifying power to the albuminous films, and bring the edge of one of the cavities to the margin of the flame, we shall perceive that both the *direct* and the *complementary* colours are formed at the very edge, the complementary ones appearing just when the direct ones have disappeared, by the withdrawal of the edge from the flame.

As the colours therefore are produced solely by the edges of the cavities, their intensity must, *cæteris paribus*, depend on the smallness of the cavities, or the number of edges which occur in a given space. When we succeed in forming an uniform film in which the cavities are like a number of minute points, the phenomena are peculiarly splendid and we are enabled to study them with greater facility. When the edges of these cavities are seen by an achromatic microscope, and in direct light, neither the direct nor the complementary colours are visible; but if we gradually withdraw the lens from the cavities a series of beautiful phenomena appear. When the vision first becomes indistinct both the direct and the complementary colours appear at the same time, specks of the *complementary red* alternating with brighter specks of the *direct green* light. By increasing the distance of the lens from the cavities, the complementary specks become less and less visible, and we see only the direct green light.

In order to study these phenomena by observing the action of a *single* edge upon light, and to ascertain the effect of an edge when there were no prismatic edges to refract, and no internal surface to reflect light, I conceived the idea of immersing thin plates of a solid substance in a fluid of such a refractive power, that the thickness of the plates should be virtually reduced to the same degree of thinness as the film of albumen between the plates of glass. The new substance described by Mr. Horner,† and which I shall call *nacrite*,

* Edin. Trans. vol. x. Part I. 407.

† Philosophical Transactions, 1836, p. 49.

furnished me with the means of performing this experiment. I accordingly inclosed the thinnest films of it between two plates of glass containing balsam of capivi; and I had the satisfaction of observing that the bounding edge of the plate and the fluid produced the identical direct and complementary colours above described.

The bounding edge which I selected for observation gave a *bright green* for the *direct*, and a *bright red* for the *complementary* tint. This edge appeared as a narrow distinct black line, exceedingly well defined, and of a uniform breadth like the finest micrometer wire. It consequently obstructed the incident light and produced the phenomena of diffracted fringes. These fringes, however, were modified by the peculiar circumstances under which they were produced, and exhibited in their tints both the direct and complementary colours under consideration.

When the diffracted fringes are viewed in candle-light by a lens placed at a greater distance from the diffracting edge than its principal focus, the middle of the system of fringes corresponding to the diffracted shadow of a fibre is occupied with the *direct tint*, which we shall suppose to be *green*; and on each side of this *green* shadow, as we may call it, we observe very faintly the *complementary red* tinging what are called the two first exterior fringes. This tinge of red is strongest in the first fringe within the solid edge, or within the green shadow, while it is *yellowish* in the first fringe without the green shadow. These effects are inverted if we place the lens nearer to the edge than its principal focus.

The phenomena now described appear more distinct if we take an extremely narrow piece of nacrite, having its two edges nearly in contact, and transmitting only a narrow line of light. In this case the two red fringes within the solid edge unite their tints, and become a bright red; and in like manner if we place the lens nearer the solid edges than its principal focus, the two yellow fringes will unite their tints, and become a brighter yellow band. In this last case, when the two bounding edges are still nearer each other, the united fringes, in place of being yellow, will be *green*, or the same as the direct colour.

If we bring the edges of two pieces of nacrite of equal thickness very near each other, having, as formerly, *green* for the *direct*, and *red* for the *complementary* colour, the space between the edges, or between the green bands, will be faint *red* when the lens is nearer the edges than its principal focus, and *yellow* when it is further from them; but if the edges are brought still nearer, the faint red will become brighter, and the united green bands will take the place of the yellow one.

Let us now return to our plate of *nacrite* with a single edge, having *green* and *red* for the two tints; and let us always suppose that the lens is adjusted to observe the diffracted fringes, that is, that the lens is placed at a greater distance from the diffracting edge than its principal focus. We shall also suppose that the light of the sun passing through a narrow aperture parallel to the diffracting edge is substituted for the light of a candle. Under these circumstances the central part of the system of fringes seen by light incident perpendicularly, consists of *blue**, *green*, and *yellow* light, constituting, as it were, the shadow of the edge, the blue light being on the same side as the plate of *nacrite*, and the yellow rays encroaching upon the exterior faint red band already described, the other red band next the blue being more distinctly seen. If we now incline the incident ray to the plate of *nacrite* more than 90° , the faint red band next the yellow gradually becomes brighter, while the other bands become fainter; and at the boundary of light and darkness all the other bands disappear except this *red* one, which is the *complementary* colour to the *green*, (produced by the union of the *blue*, *green*, and *yellow* bands), and the colour which is seen upon the dark space next the edge of the flame, as described by Dr. Young. If we, on the other hand, incline the incident ray in an opposite direction, so that it forms with the plane of the plate a less angle than 90° , the *red* band next the blue will now become brighter; and at the boundary of light and darkness, when all the other bands have disappeared, the *red* band will afford the complementary colour to the *green*.

As the edge of the plate of *nacrite* is rough and unpolished, and accurately perpendicular to the parallel faces, there are no reflected nor refracted pencils, whose combinations with one another, or with the direct rays, can be employed to account for the complementary colours. The phenomena of mixed plates, indeed, are cases of diffraction when the light is obstructed by the edge of very thin transparent plates placed in a medium of different refractive power. If the plate were opaque the fringes would be exactly those which have been so often described, and explained by the principle of interference. But owing to the *transparency* of the plate, fringes are produced within its shadow; and owing to the *thinness* of the plate, the light transmitted through it and retarded, interferes with the partial waves which pass through the plate and with those which pass beyond the diffracting edge with undi-

* Owing to the small quantity of blue rays in candle-light the blue almost disappears in it.

minished velocity, and modifies the usual system of fringes in the manner which we have described.

As the plate of nacrite diminishes in thickness, or as the fluid in which it is immersed approaches to it in refractive density, the central coloured bands, whose union constitutes the *direct* tint, will diminish in number, and descending gradually in the scale will finally disappear when the retardation produced by the plate does not perceptibly alter the phase of the ray. When the plate, on the other hand, increases in thickness, or the fluid diminishes in refractive power, the central bands will become closer and more numerous, and will finally resemble the fringes within the shadow of the ordinary system.

When the plate of nacrite is thicker at one place than another by the partial removal of a parallel film, the edge where the increase of thickness takes place produces exactly the same phenomena as the edge of the film that is removed, or of the film that is elevated above the general surface, and hence we are led to look for the phenomena of mixed plates in minerals, such as *sulphate of lime* and *mica*, where a plate of two different thicknesses can be easily obtained. I have accordingly discovered the phenomena of mixed plates distinctly exhibited in sulphate of lime and mica.

A more splendid exhibition of these colours is seen when a stratum of cavities of extreme thinness occurs in sulphate of lime. I have observed such strata repeatedly in the gypsum from Mont-martre; but they are most beautiful when the stratum has a circular form. In this case the cavities are exceedingly thin at the circumference of the circle, and gradually increase in depth towards the centre, so that we have a series of edges increasing in thickness towards a centre; the very reverse of a mixed plate, such as a film of albumen pressed between two convex surfaces. The system of rings is therefore also reversed, the highest order of colours being in the centre, while the lowest are at the circumference of the circular stratum. In many strata of cavities, such as the one which I have engraven in my paper on the new fluids in minerals,* the cavities are too deep to give the colours of mixed plates.

Another example of the colours of mixed plates in natural bodies occurs in specimens of mica, through which titanium is disseminated in beautiful flat dendritic crystals of various degrees of opacity and transparency. In these specimens the titanium is often disseminated in grains, forming an irregular

* Edinburgh Transactions, vol. x. Plate II. fig. 33.

surface. The edges of these grains, by retarding the light which they transmit, produce the direct and complementary colours of mixed plates in the most perfect manner, the tints passing through two orders of colours, as the grains of titanium increase in size towards the interior of the irregular patch. I have observed another example of these colours in the deep cavities of topaz, from which the fluids have either escaped, leaving one or both of the surfaces covered with minute particles of transparent matter, or in which the fluids have suffered induration.

*Allerly by Melrose,
October 18th, 1837.*

XXXV. *Sparks obtained from the secondary coil after the current being made to pass through water. In a letter to the Editor. By W. H. HALSE, Esq.*

Sir,

There is a fact connected with voltaic electricity which I believe has never as yet been published, and as it proves the intensity of the secondary current in a remarkable manner perhaps you may think this letter worthy a place in your next number of "the Annals."

It is well known that if we separate the two extremities of the secondary coil at the same moment that one of the primary wires of a shock apparatus is disconnected with the battery, that a spark will be visible on the *secondary* wire as well as on the *primary* wire, but in this case *metallic contact of the secondary wires has hitherto been considered necessary to produce the effect.* During a course of experiments in which I have lately been engaged, I imagined that the secondary current was sufficiently intense to give a spark after passing through water containing a very minute portion of common salt. I accordingly put my revolving apparatus to work using only one pair of cylinders both for causing the revolutions, shocks, and sparks. I then placed one end of the *secondary* wire in a glass of water, and about one inch from it I placed a file in a perpendicular position; the other end of the *secondary* coil I drew up and down this file. The revolving apparatus was going at the rate of 900 rounds per minute and gave two shocks each revolution. *Immediately I touched the file with the secondary wire I observed a spark, and by continuing the motion of the wire on the file I obtained them by scores.* Those who are not in possession of a revolving appa-

ratus or a contact breaker may perform the experiment in the following manner: affix to one of the plates of the battery a file, and to the other plate affix one end of the *primary* wire coil; then insert one end of the *secondary* coil in a glass of water containing a very small quantity of common salt or sulphuric acid, and place a file in the same glass about one inch distant from the immersed wire. Now let an assistant draw the other end of the *primary* wire across the file attached to the battery, whilst the operator draws the other end of the *secondary* wire across the dry part of the file in the glass. *Sparks will soon be perceived on this latter file and a portion of the water will be decomposed.* By keeping the file and the wire one inch or more distant from each other in the water, it is evident that the current has to pass through that space of water previous to obtaining the spark, thereby proving THAT METALLIC CONTACT OF THE SECONDARY WIRES NOT TO BE NECESSARY. I have no doubt that a spark could be taken after the current passing through one's body; but this experiment I do not like to try, neither can I get any of my friends to try it, my shock apparatus being very powerful. The battery I used was composed of cylinders immersed in a two quart pot, sulphate of copper being in contact with the copper, and a solution of common salt in contact with the zinc.

The size of the spark is much increased by increasing the number of cylinders which perhaps is unnecessary for me to mention. With ten pairs I have obtained very brilliant sparks. When the spark is to be produced from one pair the experiment should be performed in the dark. If these few lines should meet your approbation sufficient for their insertion you shall again hear from me, having several things to communicate which I believe would prove interesting to your readers; particularly a method how to increase the intensity of the coils, and also a new theory to explain why a small pair of plates, introduced into a circle, brings the action of the whole battery to that standard, De la Rives explanation of it being in my opinion unsatisfactory. Can you recommend me a recently published work which treats principally of the physiological effects of electricity.

I am, Sir,

Yours respectfully,

WILLIAM H. HALSE.

Brent, near Ashburton,
Nov. 27th, 1839.

XXXVI. *An Account of some experiments made for the purpose of ascertaining how far Voltaic Electricity may be usefully applied to the purpose of working in metal.*
By Mr. THOMAS SPENCER.

Prefatory.

Having made known, about three months ago, at a meeting of the Liverpool Polytechnic Society, that I intended to have brought the subject of the following paper before the British Association at its Birmingham meeting, I deem it a duty I owe myself,—and perhaps the public,—to state the reasons why I have not done so.

About a month previous to the meeting, I wrote to Professor Phillips, the general secretary, informing him of this, and two other papers I was desirous of laying before the Association at its next meeting, and requesting to know what forms were necessary to enable me to do so.

I received a very obliging answer in return, intimating that two of my papers would be read at the Chemical Section; but the one which is the subject of the following paper would be read at the Mechanical one, as it was deemed a portion of the process related to that science; also, that in the event of my non-attendance they would be read in my absence, by forwarding them to the secretaries of the different sections, as he would make notes to that effect.

Nothing could be more satisfactory. I, however, went to Birmingham on the Monday of the week of meeting, and immediately paid my subscription for the ensuing year, which entitled me to all the privileges of a member,—including that of reading papers, if so disposed.

Having satisfactorily completed my business at the Chemical Section, I at once proceeded to the Shakspeare Rooms, where the Mechanical one held its meetings, and inquired for the secretary. I was shown to a Mr. Carpmall. After intimating my business I inquired when I should be called on, that I might be in readiness. I was told that a note had already been made of it, and to hold myself in readiness on Thursday morning, as my paper would be called on first. With this I was perfectly satisfied; but on Wednesday, when the papers for the following day, as is usual, were announced, I could not find my name or paper in the list. I at once addressed a note to the secretary, thinking he had forgotten our previous arrangement and reminding him of it. I attended next morning at the appointed time, prepared, if called on; but on entering the committee-room, I was informed by

Mr. Carpmall that my paper could not be brought forward, giving as a reason, that "I was quite unknown." On asking what this meant, I was told that I was a man of no scientific reputation, and more especially unknown to himself and the acting president of the section, Dr. Lardner; also, that there were so many important papers to be brought forward by men of acknowledged reputation, that there was no chance for me.*

When the section had closed its labours for the day, seeing Dr. Lardner in the room, I mentioned to him, as president of the section, the arrangements that had been entered into with me by the secretary respecting my paper. Before he heard more than a few words, he told me he had nothing whatever to do with it, and haughtily turned away, adding something about valuable time.

Under these circumstances I could not bring forward the paper: but, had it not been for the unavoidable absence of Professor Phillips, I am quite sure the engagements entered into with me would have been kept.

In conclusion I may add, I can find no fault with arrangements that might be made by any Society to select such subjects as a Committee might deem proper to be laid before its members and the public, as otherwise much valuable time is often likely to be lost,—it requiring no small portion of scientific learning to be acquainted with all that *has* been done on most subjects, and without this knowledge we are too apt to stumble on what may have been years before the public. But in the instance I have related, neither the Committee nor Secretary were aware of the views I had taken of the subject I was desirous of illustrating, as they never once asked to look over my paper. Had they done so, it would have been at their service.

It is two years since I began to experimentalize on this subject. I *then* made mention of it to a few friends, (some of whom are connected with the public press in Liverpool,) but strictly enjoined them not to make it public until the experiments were matured. At the same time I showed some results obtained by this process. About four months ago a

* I may state that on the day in question, a gentleman was allowed to occupy the section by a description of the method he had adopted to cure his chimney from smoking. I mention this,—not because it was unimportant,—but because it had not even been announced. The alleged reason was that the section were on the subject of smoky chimneys. However, I concluded he was a man of known scientific reputation,—although I have since forgotten his name.

paragraph appeared in the *Athenæum*, stating that Professor Jacobi, of St. Petersburg, had received a grant of money from the Emperor, to enable him to make experiments on engraving by galvanism, as he had been enabled to preserve fine lines in relief by this principle.* I accordingly concluded that he was engaged in experiments analagous to my own: but having gone much farther than merely producing lines in relief, I at once made it public, and showed specimens of the results I had obtained in different experiments.—This was done at a meeting of the Liverpool Polytechnic Society; some of the members of which *then* spoke to their knowledge of my having been engaged on this subject for a considerable period.

I am not aware that Professor Jacobi has made his process in any way public: but if I am to judge from some specimens I saw a few days ago in Birmingham, in the model room, produced by it, I should be inclined to think that he has made small progress in this subject,—one of the specimens being a plate of copper precipitated on another, which he has been unable to get off; the printed description stating that, “through some particular circumstance they adhere together.” It will be seen, in the course of the following experiments, that I early arrived at this point which has also been easily and completely surmounted.†

I entertain no very sanguine notions as to the future *general* application of this method of operating upon the metals more especially copper. This must be entirely left to the practical engraver and printer.

* See a notice of Jacobi's experiments at p. 507, vol. 3, of these Annals. Edrr.

† I cannot help finding some fault with the mode adopted by the professor (or, it may be, injudicious friends), of announcing his discoveries. About twelve months ago, I imagined that I had discovered a method of obviating, by simple means, the great difficulty in the construction of electro-magnetic Engines; but, on seeing several paragraphs from time to time in the newspapers, professing to be copied from letters received from St. Petersburg, stating that Dr. Jacobi *had succeeded* in constructing engines of considerable power, on this principle, I at once gave up my researches on the subject, thinking the thing already done. It is only a few weeks since a statement appeared (in a Liverpool paper,—on the authority of a letter received by Professor Wheatstone from Dr. Jacobi,) that he *had* an electro-magnetic machine, of forty-horse power, *at work* on the Neva; but, since, it appears another letter states he had *hoped* only to have such an engine by this period, and still hopes by next year to accomplish his object—but has not as yet succeeded with one of three-horse power.

The question with them will be,—Is it cheaper and better than the methods in common use? It may now be answered—Give it a fair trial: the way is pointed out—practice will no doubt enable you to improve upon the methods which suggested themselves during the experimental investigation detailed in the following pages, and most probably may realise an extended field of practical utility for the peculiar mode of operation which has been the result.

I feel assured, however, that, in the arcana of many trades and branches of art, this process will be found an important addition—supplying as it does a means of producing a cast, or a die, in hard metal, *without the agency of heat or pressure*, and in extreme perfection and well-defined sharpness. Nor, (I need hardly observe) is its application confined to copper only.

In addition to the applicability of this process, in procuring exact fac similes of *coins*, or *medals*, with all the lineal sharpness of the original, perfect copies may be obtained of bronzed *figures*; nor do they require chasing when taken out—nor do I apprehend inconvenient limitation as regards their size.

Assuming it to be advantageous to publishers of music to have their plates *in relief*, by this process they will be enabled, in the original engraving, to have them so.

I have seen nothing in wood engraving that might not be produced in copper, in relief, by this means; the chemical plates might, possibly, require retouching to a small extent, but, with careful manipulation, twenty or thirty such plates might be taken from one mould.

I may mention that the advantage of being able to produce a given effect from a plate in relief would be very considerable, as ten printed impressions may frequently be taken, in the time occupied in producing one by the ordinary method from a copper-plate. Plates *in relief* might also frequently be printed off in the body of the work—which, in point of economy, would be a very considerable advantage.

In the formation of that important implement in the manufacture of printing types—the matrix or mould,—advantages in the adoption of this operation appear to present themselves. And I am assured by the printers of this pamphlet, that it gives fair promise to supply several important desiderata in the art of printing, and in its attendant operations,—more particularly in the stereotype process.

In general,—I feel convinced that it exhibits many promising indications of utility, should no obstacles in a pecuni-

ary point of view present themselves, on occasion of attempts to extend the application of the discovery.

In the following paper I have detailed a few of the most illustrative experiments made during the investigation, trusting that they might be found interesting, not only to the general reader, as illustrating the progress of discovery, but to the future experimentalist, in pointing out to him the methods that have best succeeded, as well as those he ought to avoid. In all cases I deem details of chemical experiments essentially necessary; as one *apparent* trifle omitted, is more than likely to retard the labours of the future practitioner.

Having made many experiments on a larger scale than those detailed, since writing the following paper, I shall, at its conclusion, detail the methods to be adopted under different circumstances.

First:—*To engrave in relief on a plate of copper.*

Second:—*To deposit a voltaic copper-plate, having the lines in relief.*

Third:—*To obtain a fac-simile of a medal (reverse or obverse), or of a bronze cast.*

Fourth:—*To obtain a voltaic impression from plaster or clay.*

Lastly:—A method of *multiplying the number of already engraved copper-plates.* This last promises to be of vast import,—more especially in the Potteries, as there they require, in many instances, eight or ten copper-plates of a similar pattern. By the method I shall point out, I can see no reason why they should not be able to multiply them *ad infinitum.*

I shall also give some rules for the management of the apparatus, which my experience of the process has suggested.

When I have done this, I shall then have laid before the public the result of many an anxious—and, I may add, pleasant—hour: each experiment requiring a considerable lapse of time for its development; but when attended with success—no words of mine can convey the pleasurable feelings coupled with such a result.

I have been led on, by the fond hope that the present simple discovery may be the foundation of a vast structure of Synthetic Chemistry, which is perhaps destined, (at no distant date), to imitate, for the uses of humanity, all the most wonderful, but apparently complicated, elaborations of Nature.

PAPER.

Notice given May the 8th—read September the 12th, 1839.

HENRY BOOTH, Esq. *President*, in the Chair.

In the paper I have now the honour to lay before the Society, I do not profess to have brought forward a perfect invention. My only object is to point out a means by which, I hope, practical men may ultimately be enabled to apply a great and universal principle of nature to the useful and ornamental purposes of life. In this I may be considered sanguine,—an error, I am aware, too often fallen into by those, who, like myself, imagine they have discovered an useful application of an important principle; but however this may fall out, I now proceed to lay an account of its results, successful and unsuccessful, before the members and the public,—previously stating, however, that all my first experiments were made on a small scale; a method of procedure attended with many advantages to the experimentalist himself, but having its disadvantage when laid before the public. In this first respect, the chemical experimenter has a decided advantage over the mechanical one; the success of his experiment, when tried on a small scale, doubly guarantees its success, if conducted on a still larger—with mechanical results I believe in most instances it is the reverse. But, when the chemist produces his microscopic proofs, the public are generally slow to believe that such minute appearances should warrant him in coming to any general conclusion.

In September, 1837, I was induced to try some experiments in Electro-chemistry, with a single pair of plates, consisting of a small piece of zinc and an equal sized piece of copper, connected together with a wire of the latter metal. It was intended that the action should be slow; the fluids in which the metallic electrodes were immersed were in consequence separated by a thick disc of plaster of paris. In one of the cells was sulphate of copper in solution, in the other a weak solution of common salt. I need scarcely add that the copper electrode was placed in the cupreous solution. I mention this experiment, briefly,—not because it is *directly* connected with what I shall have to lay before the Society, but because, by a portion of its results, I was induced to come to the conclusions I have done in the following paper.* I was desirous that no

* The experiment here alluded to was to determine a most important point—and as it has an intimate connexion with the future

action should take place on the wire by which the electrodes were held together. To obtain this object I varnished it with sealing-wax varnish:—but, in so doing, I dropped a portion of it on the copper that was attached. I thought nothing of this circumstance at the moment, but put the experiment in action.

The operation was conducted in a glass vessel; I had consequently an opportunity of occasionally examining its progress. When, after the lapse of a few days, metallic crystals had covered the copper electrode,—*with the exception of that portion* which had been spotted with the drops of varnish, I at once saw that I had it in my power to guide the metallic deposition in any shape or form I chose, by a corresponding application of varnish, or other non-metallic substance.

I had been long aware of what every one who uses a sustaining galvanic battery with sulphate of copper in solution must know,—that the copper plates acquire a coating of copper from the action of the battery; but I had never thought of applying it to a useful purpose before. My first essay was with a piece of thin copper plate having about four inches of superficies, with an equal sized piece of zinc, connected together with a piece of copper wire. I gave the copper a coating of soft cement, consisting of bees' wax, resin, and a red earth—Indian or Calcutta red. The cement was compounded after the manner recommended by Dr. Faraday in his work on chemical manipulation; but with a larger proportion of wax. The plate received its coating while hot. On cooling, I scratched the initials of my own name rudely on the plate, taking special care that the cement was quite removed from the scratches, that the copper might be thoroughly exposed. This was put in action, in a cylindrical glass vessel about half filled with a saturated solution of sulphate of copper. I then took a common gas glass, similar to that used to envelope an

application of the results detailed in this paper, I may be excused in briefly alluding to it here. In fact no experiment can be made with any certainty, without keeping its results in view.

In September, 1837, at the Liverpool meeting of the British Association, a clever young demonstrator (Dr. Bird, of London) asserted that in an experiment he had made, he had obtained crystals of pure copper *without* the intervention of a metallic nucleus to commence with. I doubted this at the time, as it was opposed to all former experience. However, I made several very careful experiments, following Dr. Bird's plan in all he stated; then varied them in order to give it every chance of success. The result was that *no metallic crystallization will take place*, unless a metallic or metalliferous nucleus be present.

Argand burner, and filled one end of it with plaster of paris, to the depth of three-quarters of an inch. In this I put some water, adding a few crystals of sulphate of soda to excite action, the plaster of paris serving as a partition to separate the fluids, but sufficiently porous to allow the electro-chemical fluid to permeate its substance.

I now bent the wires in such a form that the zinc end of the arrangement should be in the saline solution, while the copper end should be in the cupreous one. The gas glass, with the wire, was then placed in the vessel containing the sulphate of copper.

It was then suffered to remain, and in a few hours I perceived that action had commenced, and that the portion of the copper rendered bare by the scratches was coated with the pure bright deposited metal, whilst all the surrounding portions were not at all acted on. I now saw my former observations realised;—but whether the deposition so formed would retain its hold on the plate, and whether it would be of sufficient solidity or strength to bear working if applied to a useful purpose, became questions which I now endeavoured to solve by experiment.

It also became a question whether—should I be successful in these two points—I should be able to produce lines sufficiently in relief to print from. This latter appeared to depend entirely on the nature of the cement or etching-ground I might use.

This last I endeavoured to solve at once. And (I may state) this appeared to be the principal difficulty; as my own impression then was, that little less than one-eighth of an inch of relief would be requisite.

I then took a piece of copper, and gave it a coating of a modification of the cement I have already mentioned, to about one-eighth of an inch in thickness; and, with a steel point, endeavoured to draw lines in the form of net-work, that should entirely penetrate the cement, and leave the surface of the copper exposed. But in this I experienced much difficulty, from the thickness I deemed it necessary to use; more especially, when I came to draw the cross lines of the net-work. When the cement was soft, the lines were pushed as it were into each other; and when it was made of harder texture, the intervening squares of the net-work chipped off the surface of the metallic plate. However, those that remained perfect I put in action as before.

In the progress of this experiment, I discovered that the solidity of the metallic deposition depended entirely on the weakness or intensity of the electro-chemical action, which I

found I had in my power to regulate at pleasure, by the thickness of the intervening wall of plaster of paris, and by the coarseness and fineness of the material. I made three similar experiments, altering the texture and thickness of the plaster each time, by which I ascertained that if the plaster partitions were *thin* and *coarse*, the metallic deposition proceeded with great *rapidity*, but the crystals were friable and easily separated; on the other hand, if I made the partition thicker, and of a little finer material, the action was much slower, and the metallic deposition was as solid and ductile as copper formed by the usual methods,—indeed, when the action was exceedingly slow, I have had a metallic deposition apparently much harder than common sheet copper, but more brittle.

There was one most important, (and, to me, discouraging) circumstance, attending these experiments, which was, that when I heated the plates, to get off the covering of cement, the meshes of copper net-work invariably *came off with it*. I at one time imagined this difficulty insuperable, as it appeared to me that I had cleared the cement entirely from the surface of the copper I meant to have exposed,—but that there was a difference in the molecular arrangement of copper prepared by heat, and that prepared by voltaic action, which prevented their chemical combination. However, I then determined, should this prove so, to turn it to account in another manner, which I shall relate in the second portion of this paper.

I then occupied myself for a considerable period in making experiments on this latter section of the subject.

In one of them I found, on examination, a portion of the copper deposition, which I had been forming on the surface of a coin, adhered so strongly that I was quite unable to get it off,—indeed, a chemical combination had apparently taken place. This was only in one or two spots, on the prominent parts of the coin. I immediately recollected that on the day I put the experiment in action, I had been using nitric acid, for another purpose, on the table I was operating on, and that in all probability the coin might have been laid down where a few drops of the acid had accidentally fallen. I then took a piece of copper, coated it with cement, made a few scratches on its surface until the copper appeared, and immersed it for a short time in dilute nitric acid, until I perceived, by an elimination of nitrous gas, that the exposed portions were acted upon sufficiently to be slightly corroded. I then washed the copper in water, and put it in action as before described. In forty-eight hours I examined it, and found the lines were entirely filled with copper. I applied heat, and then spirits

of turpentine, to get off the cement ; and, to my satisfaction, I found that the voltaic copper had completely combined itself with the sheet on which it was deposited.

I then gave a plate a coating of cement, to a considerable thickness, and sent it to an engraver ; but when it was returned, I found the lines were cleared out so as to be wedge-shaped, or somewhat in the form of a ∇ , leaving a hair line of the copper exposed at the bottom, and a broad space near the surface ; and where the turn of the letters took place, the top edges of the lines were galled and rendered rugged by the action of the graver. This, of course, was an important objection ; which I have since been able to remedy, in some respects, by an alteration in the shape of the graver, which should be made of a shape more resembling a narrow parallelogram than those in common use,—some engravers have many of their tools so made. I did not put this plate in action, as I saw that the lines, when in relief, would have been broad at the top and narrow at the bottom. I took another plate, gave it a coating of the wax, and had it written on with a mere point. I deposited copper on the lines, and afterwards had it printed from.*

I now considered part of the difficulties removed : the principal one that yet remained was, to find a cement or etching-ground, the texture of which should be capable of being cut to the required depth,† without raising (what is technically termed) a *burr*, and, at the same time, of sufficient toughness to adhere to the plate, when reduced to a small isolated point, which would necessarily occur in the operation which wood-engravers term cross-hatching.

I tried a number of experiments with different combinations of wax, resins, varnishes, and earths, also with metallic oxides,—all with more or less success.

The one combination that exceeded all the others in its texture, having nearly every requisite (indeed I was enabled to polish the surface nearly as smooth as a plate of glass), was principally composed of virgin wax, resin, and carbonate of lead—the white lead of the shops.

With this compound I had two plates, 5 inches by 7, coated over, and portions of maps cut on the cement, which I

* This plate was shown, and also specimens of printing from it.

† I have since learnt, from practical engravers, that much less relief is necessary, to print from, than I had deemed indispensable ; and that on becoming more familiar with the cutting of the wax-cement, they would be enabled to engrave in it with great facility and precision.

had intended should have been printed off and laid before the British Association at its meeting. I applied the same process, to these, as to others—dipping them in dilute nitric acid before putting them in action: indeed I suffered them to remain about ten minutes in the solution. I then put them into the voltaic arrangement. The action proceeded, slowly and perfectly, for a few days,—when I removed them. I then applied heat, as usual, to remove the cement,—when all came away as in a former instance; the voltaic copper peeling off the plate with the greatest facility. I was much puzzled at this unexpected result, but, on cleaning the plate, I discovered a delicate tracing of *lead*, exactly corresponding to the lines drawn on the cement previous to the immersion in the dilute acid. The cause of this failure was at once obvious; the carbonate of lead I had used to compound the etching-ground had been decomposed by the dilute nitric acid, and the metallic lead thus set free had deposited itself on the exposed portions of the copper-plates, preventing the voltaic copper from chemically combining with the sheet copper. I was now obliged with regret to give up this compound—although, under other circumstances, I have no doubt it may be rendered available.—I adopted another, consisting of bees' wax, common whiting, resin, a small portion of gum, and plaster of paris. This seems to answer the purpose tolerably—though I have no doubt, by an extended practice, a better may still be obtained.

I now proceed to the second, and I believe the most satisfactory, portion of the subject. Although I have placed these experiments last, they were made simultaneously with the others already described; but, to render the subject more intelligible, I have placed them thus.

I have already stated that I was desirous of executing metallic ornaments by this means, in either cameo or intaglio; but, being well aware of the apparent natural law which prevents metallic deposition by voltaic electricity, *without* the presence of a metallic body, I perceived, in consequence, its uses, if any, would be extremely limited, as, whatever ornament it might produce, it would only be by adhering to the condition of a metallic mould.

I accordingly determined to make my first experiment on a very prominent copper medal. I placed it in a voltaic circuit as already described, and deposited a surface of copper on one of its sides to about the thickness of a shilling. I then proceeded to get the deposition off. In this I experienced some difficulty, but ultimately succeeded. On examination with a magnifying glass, I found every line was as perfect as the coin

was from which it was taken. I was then induced to use the same piece again, and let it remain a much longer time in action, that I might have a thicker and more substantial mould. I accordingly put it again in action, and let it remain until it had acquired a much thicker coating of the metallic deposition; but when I attempted to remove it from the medal, I found I was unable. It had, apparently, completely adhered to it.

I had often practised, with some degree of success, a method of preventing the oxidation of polished steel, by slightly heating it until it would melt virgin wax; it was then wiped, apparently, completely off,—but the pores of the metal became impregnated with the wax.

I thought of this method, and applied it to a copper coin.

I first heated the piece,—applied wax—and then wiped it so completely off, that the sharpness of the coin was not at all interfered with. I proceeded as before, and deposited a thick coating of copper on its surface, after the lapse of a few days. When I wished to take it off, I applied the heat of a spirit-lamp to the back; when a sharp crackling noise took place, and I had the satisfaction of perceiving that the coin was completely loosened. In short, I had a most complete and perfect copper mould of a halfpenny.

I have since taken some impressions from the mould thus taken; and, by adopting the above method with the wax, I get them out with the greatest ease.

I was now of opinion that this latter method might be applied to engraving much better than the method described in the first portion of this paper. Being aware that copper in a voltaic circuit deposited itself on lead with as much rapidity as on copper, I took a silver coin, and put it between two pieces of clean sheet lead, and placed them under a common screw press. From the softness of the lead, I had a complete and sharp mould of both sides of the coin. I then took a piece of copper wire,—soldered the lead to one end—and a piece of zinc to the other, and put them into the same voltaic arrangement I have already described. I did *not*, in this instance, *wax* the mould, as I felt assured that the deposited copper would easily separate from the lead, by the application of heat,—from the different expansibility of the two metals.

In this result I was not disappointed. When the heat of a spirit-lamp was applied for a few seconds to the lead, the copper impression fell easily off. So complete do I think this latter portion of the subject, that I have no hesitation in asserting that fac-similes of any coin or medal, no matter of what size, may be readily taken, and as sharp as the original.

To further test the capabilities of this method, I took a piece of lead plate, and stamped some letters on its surface to a depth sufficient to print from when in relief. I deposited copper on it, and found it came easily off.

I now come to the conclusion of my experiments on this subject. As I stated at first, my object was to deposit a metallic surface on a model of clay, or other *non-metallic* body,—as, otherwise, I imagined the application of this principle would be extremely limited. I made many experiments to achieve this result, which I shall not detail, but content myself with describing that which was ultimately the most successful.

I took two models of an ornament, one made of clay, and the other of plaster of paris; soaked them for some time in linseed oil; took them out, and suffered them to dry—first getting the oil clean off the surface. When dry, I gave them a thin coat of mastic varnish. When the varnish was as nearly dry as possible—but *not thoroughly so*, I sprinkled some bronze powder on that portion I wished to make a mould of. This powder is principally composed of mercury and sulphur. I had, however, a complete metallic coating on the surface of my model, by which I was enabled to deposit a surface of copper on it, by the voltaic method I have already described. I have also gilt the surface of a clay model with gold leaf, and have been successful in depositing the copper on its surface. There is likewise another, and (as I trust it will prove) a simpler method of attaining this object, but as I have not yet sufficiently tested it by experiment, I shall take another opportunity of detailing the method.

[At the close of the paper, several specimens of coins and medals—some of them in the act of formation by the voltaic process—were exhibited to the members.]

ADDENDA.*

TO ENGRAVE IN RELIEF ON A PLATE OF COPPER.

Take a plate of copper, such as are in use among engravers. It is not essential that it should be highly polished.

Have a piece of copper wire neatly soldered to the back part of it, and then give it a coating of either of the cements

* *Note*—By this process, iron castings that are required to be preserved from the weather may have a coating of copper given to them, of any requisite thickness.

already mentioned. This is best done by heating the plate as well as the wax ; or, to level the wax after it has had a coat, hold the back part of the plate over a charcoal fire, or spirit-lamp,—taking care to hold it level.

Then write, or draw the design, on the wax, with a black-lead pencil or a point. The wax must now be cut through with a graver, or a steel point,—taking special care that the copper *is exposed on every line*.

It must now be immersed in dilute nitric acid—say, three parts water to one acid. It will be at once seen whether it is strong enough, by the green colour of the solution, and the bubbles of nitrous gas eliminated. Let it remain long enough to allow the exposed lines on the plate to be *slightly* corroded ; that the wax (which gets into the pores of the copper during the heating process), may be thoroughly got rid of. Practice will determine this, better than any rules.

The plate is now ready to be placed in the voltaic apparatus (see Engraving, p. 3.) After the voltaic copper has been deposited in the lines engraved in the wax, the surface of the formation will be found to be rough, more or less, according to the quickness of the action. To remedy this, rub the surface with a piece of smooth flint or pumice-stone, with water. Then heat the plate, and wash off the wax groundwork with spirits of turpentine and a brush. The plate is now ready to be printed from at an ordinary press.

TO DEPOSIT A SOLID VOLTAIC PLATE, HAVING THE LINES
IN RELIEF.

Take a plate of copper, lead, silver, or type-metal, of the required size, and engrave in it, to the depth requisite to print from, when in relief.

Contrary to ordinary engraving, the lines must be *flat* at the bottom, and as nearly as possible of the same depth. When so engraved (should the plate be copper or silver), heat it, and then apply a little bees' wax (what is termed virgin wax is preferable) mixed with a very small proportion of spirits of turpentine ; and give the plate a coating of it. It may be laid on in a lump ; and the heat of the plate should be sufficient to melt it. When on the eve of cooling, the plate should be wiped clean, and all the wax taken off,—as sufficient will have entered the pores of the plate, to prevent the voltaic copper from adhering.

Then solder a piece of copper wire.

The plate must now receive a couple of coats of thick varnish on the back and edges (a preparation of shell-lac and alcohol does very well). I prefer, if the plate is large, to imbed it with plaster of paris or roman cement, in a box the size of the plate, allowing the wooden edge of the box to project just as much above the surface of the plate, as you wish the thickness of the voltaic one to be. (Care must be taken, to keep the engraved surface of the plate clean).

It is now ready to be placed in the apparatus to be deposited on.

Should the plate be lead,—or, what is still better, type-metal,—the preparation of wax does not require to be given to the plate, as, when it is deposited on to the given thickness, applying heat is sufficient to loosen the plates.

TO PROCURE FAC-SIMILES OF MEDALS, &c.

This may be done by two different methods ; the one, by depositing a *mould* of the voltaic metal on the face of the medal, (having first heated it, and applied wax), and then depositing the metal (by a subsequent operation) in the mould so formed.

But the more ready way is, to take two pieces of milled *sheet* lead, (cast-lead not being equally soft), having surfaces perfectly clean and free from indentation. Put the medal between the two pieces of lead, subjecting the whole to pressure in a screw-press.† A complete mould of both sides is thus formed in the lead, showing the most delicate lines perfect (in reverse). Twenty—or even a hundred—of these may be so formed on one sheet of lead, and are deposited by the voltaic process with equal or greater facility ; as, the more extensive the apparatus, the more regularly and expeditiously does the operation proceed.—Those portions of the surface of the lead, where the moulds do not occur, may be varnished,

* It may be necessary to note, that the voltaic mould will also require the application of the wax.

† A common copying-machine will serve the purpose, for a small medal, not having much relief.

Should the medal be large, and in bold relief, it would be better to have a small portion of the lead cut out, or turned in a lathe, so that the medal might (to a certain extent) fit into the lead before being pressed up : this will prevent injury of the medal, and give a rim to the fac-simile.

to neutralize the voltaic action ; or, (a whole sheet of copper being deposited), the voltaic medals may afterwards be cut out.

A piece of wire must now be soldered neatly to the back of the leaden plate ; it is then ready to be put in action.

[This applies to the formation of one side of a medal only. It requires extremely careful manipulation to form both sides ; and as I think there may be a better method than the one I have hitherto adopted, I defer stating it until I have obtained the result of an experiment now in operation.]

A VOLTAIC IMPRESSION FROM A PLASTER OR CLAY MODEL.

This process is partially described in a preceding page ;* in addition to which I may state, that when the plaster or clay ornament is gilt with gold leaf, or bronzed, a copper wire should be attached to it, by running through from the back, until the point appears above the front surface,—or level with it will be sufficient. The other end must then be attached to the binding screw connecting it with the zinc, in all respects similar to any of the foregoing methods.

TO OBTAIN ANY NUMBER OF COPIES FROM AN ALREADY ENGRAVED COPPER-PLATE.

A copper-plate may be taken, engraved in the common manner—the lines being in *intaglio*. Procure an equal-sized piece of sheet-lead ; lay it on the engraved side of the plate, and put both under a *very powerful* press ; when taken out, the lead will have every line, in relief, that had been sunk in the copper.

A wood engraving may be operated on in like manner ; —as lead, being pressed into it, will not injure it.

A wire may now be soldered to the lead, then bed it in a box ; and put it into the voltaic apparatus,—when a copper-plate, being an exact fac-simile of the original, will be formed.

In this process, care must be taken that the lead is clean and bright, as it comes from the roller in the milling-process, and consequently free from any oxidation, which it soon acquires, if exposed to the atmosphere. It should be put in action as soon as possible after being taken out of the press.

* See page 263.

REMARKS

ON THE MANAGEMENT OF THE APPARATUS.*

Next to electro-magnetism, there is no branch of science that requires more dexterous manipulation than voltaic, or electro-chemistry; the most trifling film of oxidation often retarding the action of the most powerful apparatus. But, in the present instance, slow action, and simplicity of arrangement, being the predominating features, such nice attention to minutiae is not absolutely necessary,—or at least not so much so as to deter those hitherto unacquainted with the subject from practising.

In *all* cases, to ensure metallic connexion, binding-screws are preferable to cups of mercury; but, in using them, the copper wire, where the attachment is made, must be brightened with a piece of emery paper,—also the point of the screw, where it presses on the wire. In soldering the wires to the plates, let as little resin be used as possible; sal ammoniac, or dilute muriatic acid, answers the purpose much better.

In these experiments, I have invariably found an *equal sized* piece of zinc to answer best. In the construction of galvanic batteries in general, I am aware, this is a moot point with high authority; but my own practice, which has been by no means small, with batteries of every construction, has led me to the opinion that, wherever slow and equable action is required, the positive and negative electrodes should be of equal *superficial* area.—Although amalgamated zinc plates are preferable where combined intensity and continuity of action are required, they must not be used, under any circumstances, for the present purposes.—It will, likewise, be found to be essential that the *thickness* of the zinc be equal to that of the required deposition.

Let the porous bottom of the interior vessel, containing the zinc, be a little larger than either of the electrodes. I have hitherto used, for this purpose, either bottomless glass cylinders, or wooden boxes, varnished, with plaster bottoms; but I should recommend a well glazed earthenware vessel, having no bottom, but a slight rim projecting inwards, to secure the plaster. The zinc should be occasionally taken out of the arrangement, during continuance of the process, and cleansed by washing it in water; the saline solution may also be renewed.

* These observations are intended for the guidance, in the first instance, of those who are practically unacquainted with voltaic arrangements.

Crystals of sulphate of copper should be added, from time to time, to the cupreous solution; but, should the deposition require to be thick and long-continued, it will be necessary to take out the cupreous solution once or twice during the operation, and add an entirely fresh one,—as the sulphuric acid, necessarily set free after the de-oxidisement of the copper, when it predominates to any extent, prevents the required action from taking place on the copper; instead of which, a sub or di-oxide of copper is deposited, in the form of a reddish brown powder—the solution being rendered colourless. When this takes place, the plate should be taken out, and well washed in very dilute nitric acid. I have tried several methods to take up the sulphuric acid as it was set free; pure clay answers this purpose pretty well, the acid combining to a certain extent with it, and forming a sulphate of alumina, or alum, at the bottom of the vessel.

When the voltaic copper is bent, it breaks at a similar angle to cast copper; but when heated to a red heat, and slowly cooled, it assumes somewhat of the pliability of rolled sheet copper, requiring to be bent several times before breaking; should it now be beaten on an anvil, it will resume its brittleness.

It may be filed, polished, and cut with shears, in the usual manner—the surface acquiring as fine a polish as the copper in use among engravers.

Should a thick mass of metal be requisite for any practical purpose: as it would require a considerable lapse of time before it could be obtained by the voltaic process,* the back of the deposited metal may be thickened, or filled up with solder, in a manner already practised in the arts, without the slightest injury to the surface or texture of the deposited metal.

* To deposit metal equal to one-eighth of an inch in thickness, requires about eight or ten days' continuous action: the superficial extent of the deposition not being material, as regards the duration of the process.

DESCRIPTION (AND ACCOMPANYING VIEW) OF THE APPARATUS.

FIGURE 1.

FIGURE 1 is a Section of the necessary Apparatus, which may be made of any size.

(A.) An earthenware vessel, containing a solution of Sulphate of Copper.

(C.) An inner pan, of earthenware or wood having a plaster of paris bottom, made to fit into the interior of (A), and containing a saline or acidulous solution.

(B.) The plate to be deposited on; immersed in the cupreous solution, and having a wire (F) attached, which connects with the binding screw (E), soldered to the zinc plate (D) immersed in the saline solution.

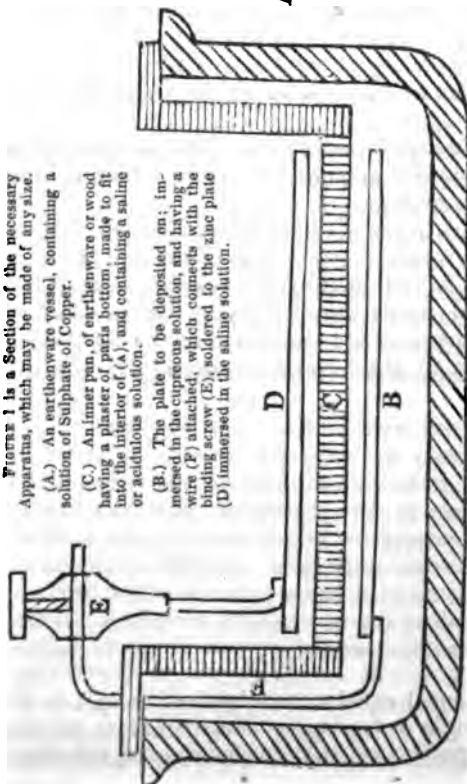


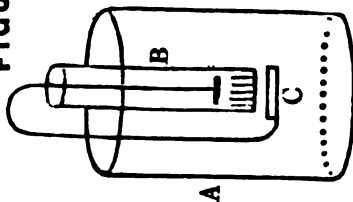
FIGURE 2.

FIGURE 2 is a more simple apparatus,—but on the same principle as Fig. 1. This is adapted for experiments on a small scale; or to take a facsimile of a single medal.

A. may represent a common drinking-glass, containing the copper solution.

B, a gas-glass, having one end closed with plaster of paris; and containing the saline solution.

C, the plate or medal, required to be deposited on, having a plate attached, to the other end of which is a piece of zinc,—the wire being bent into the requisite form.



... The above engraving has been produced (in relief) by the Electro-chemical Process described in the preceding pages;—and is the first result of that process appearing in print.

It is however by no means a fair specimen of what may be done by it.—The lines were originally cut in a sheet of soft lead, hastily, and without reference to ornamental beauty of execution. The want of a tool properly adapted to cut the lead, accurately, in the required manner, has made the lines less regular in formation than they would otherwise have been. The letters were punched into the lead, with types. The lead, thus prepared, was then put into the apparatus it illustrates, and a plate of copper deposited, the lines being in relief,—which is here printed off, accompanying the pages of the treatise.

NOTE.

We have read Mr. Spencer's paper with much pleasure and satisfaction; and must certainly express our extreme surprise at our author's extraordinary reception at the Birmingham Meeting of the British Association, *for the promotion of Science*. We have not the pleasure of knowing who Mr. Spencer is, nor what is his position in society: but no one, who reads the preceding paper, can hesitate acknowledging that he is a "scientific man." As for ourselves, we have no hesitation in stating, that Mr. Spencer's paper is, without exception, the most interesting one, on voltaic electricity, that has hitherto been presented to the notice of the British Association *for the promotion of Science*. EDIT.

ADDITIONAL INFORMATION FROM MR. SPENCER.

To the Editor of the Liverpool Mercury.

Sir,

In your last number you expressed some doubts as to the correctness of the account of my process, given in the Athenæum, and condensed from the account given in the pamphlet published by the Polytechnic Society. I have since looked over it, and find nothing absolutely incorrect, yet in the attempt to condense the details of the process, there is a turbidness which materially interferes with a proper understanding of the whole. I shall now detail to you, as briefly as possible, the method I would adopt to copy a wood engraving in copper,—and as it will apply to some other processes connected with the subject, it will, I trust, not be unacceptable to your readers.

I may premise that, but for the plasticity and perfectly unelastic property of lead, the discovery would be of but comparatively small value. Plumbers who have handled the substance for the greater portion of their lives, are astonished to find it so susceptible of pressure; on the contrary, wood engravers did not, until now, imagine their blocks would stand the pressure of a screw press on a lead surface without injury; but such is the fact in both instances. In the manner in which box wood is used for wood engravings, being horizontal sections, it will sustain a pressure of 8000lbs. without injury, provided the pressure is perfectly perpendicular.

The wood engraving being given, take a piece of sheet lead the requisite size; let its superfiice be about one-eighth

of an inch larger all round than that of the wooden block. The lead must now be planed with a common plane, just as a piece of soft wood: the tool termed by the joiner the try plane does best;—a clear bright surface is thus obtained, such as I have been unable to get by any other means. The engraved surface of the wood must now be laid on the planed surface of the lead, and both put carefully in the press; should the engraving have more than two inches of superficies, a copying press is not powerful enough. Whatever press is used, the subject to be copied must be cautiously laid in the centre of the pressure, as a very slight lateral force will in some degree injure the process. The lead to be impressed upon must rest on the iron plate of the press, as must the back part of the wood engraving; the pressure to be applied regularly, and not, as in some cases with a jerk. When the pressure is deemed complete, they may be taken out; and if, on examination, the lead is not found to be completely up, the wood engraving may be neatly relaid on the lead, and again submitted to the press, using the same precaution as before.

When the lead is taken out a wire should be soldered to it *immediately*, and put into the apparatus without loss of time, as the less it is subjected to the action of the atmosphere the better;—care should also be taken not to touch the surface with the fingers. In the pamphlet I stated the length of time usually taken to deposit the required thickness of metal;—I have been since able to abridge that period three or four fold, as I keep the solutions at a temperature of from 120 to 180 Fahrenheit. It has been suggested to me, by Mr. Crosse, of Broomfield, to keep the solutions boiling, which still further increases the rapidity of the deposition. Contrary to general chemical analogy the deposited metal is of a much superior quality to that deposited by the very slow action of a common temperature.

At the same time it must be borne in mind, that if the process is quickened by strengthening the solution in the positive cell by the addition of an acid, the metal deposited in the opposite one is of a very inferior quality; so much so as to be totally unfit for any practical purpose. Under these circumstances the deoxidizing process is not completed, the deposit being a reddish brown protoxide of copper; this last, if let remain for a few days longer, undergoes a still further change, it then becomes a black oxide of copper, such as may be used for organic analysis; and, were I to pursue this branch of chemistry, I should never resort to any other method of obtaining it.

The above process will apply to copying engraved copper-plates, or medallions.

I have also been able to obtain impressions from wood engravings by the following method. Take a piece of tinfoil the size, or thereabouts, of the engraving; place it on the engraved surface; over this place a piece of sheet India rubber, and put the whole in a press; on taking out of which it will be found the tin is thoroughly impressed into the lines of the wood. A coating of plaster of Paris must now be laid on the tin to about half an inch in thickness; when set, the whole may be taken off the wooden block. It will be found that the tin adheres to the plaster, and leaves the face of the engraving. The tin surface may now be deposited on to any required thickness. The above was tried on a coarse wood engraving. I am unable to say how it might answer for a fine one.

I have been more than once reminded of the fusible metal, that melts at a temperature of boiling water, but have had no opportunity of trying it; it might be applicable for copying wood engravings.

I have yet another method which I am in hopes will still further improve the process, but as it is not matured I shall take a future opportunity of communicating it: being a modification of the apparatus, it will require an engraving to explain it.

Yours, &c.,
THOMAS SPENCER.

XXXVII. *On the use of Voltaic Electricity. In a letter to the Rev. J. B. Reade. By MR. STURGEON.*

Westmoreland Cottage,
December 2, 1839.

My dear Sir,

During our conversation, this morning, on the subject of taking fac simile impressions, in copper, of medallions, coins, &c., by the process of Voltaism, you will remember that the idea occurred to me of giving them silver or golden surfaces, by a similar voltaic process; employing a solution of either of those metals in connexion with the *prepared* matrix, instead of a solution of copper. Turning the subject over in my mind whilst walking home, a thought struck me that a *complete* medallion of any kind of metal might easily be made by the voltaic process; or the medallion might be constructed

of different metals and in a variety of ways, which it would be difficult to imitate by any other process.

The following are some of the methods:—

Let a matrix of each side of the medallion intended to be copied be made in the usual way, by means of the alloy usually called *Newton's fusible metal*, and let the metal be about an eighth of an inch in thickness. To the back of this metal is to be soldered one end of a copper wire, and to the other end a piece of zinc, which is afterwards to be amalgamated. The metal in which the matrix is formed is now to be covered with a thin stratum of either varnish or wax, leaving bare the matrix only. The wire is also to be covered in a similar manner, and is to be bent so as to adapt the voltaic metals to their respective positions in the vessels holding the liquids employed. In a few hours the matrix will have received a coating of precipitated metal from the solution, which may be either gold or silver: the thickness of the coating will depend upon the time. When this coating is supposed to be of sufficient thickness, remove the solution of the silver or the gold, as the case may be, and replace it by a solution of the sulphate of copper, and in the course of a few days you will have a considerable thickness of copper precipitated on the silver coating in the matrix. These two metals will adhere firmly together so as to be one piece. When this young semi-medallion is removed from the matrix, it will have a copper body with a silver or a gold face. Its twin sister may be formed by proceeding in the same way, with the matrix formed from the opposite face of the original medallion, and when the process is completed the flat copper sides may be soldered neatly together, so as to form a complete medallion similar to the original one.

By a similar process a complete medallion may be formed having a gold surface on one side and a silver one on the other.

Another beautiful variation may be made by the following process. Imagine that we wanted a medallion whose *prominent* parts should be of gold, and the rest silver. The head of NEWTON, for instance, with its motto, to be of gold. Varnish with wax every other part of the matrix, and put it in galvanic action in a solution of gold. In a few hours a golden head and motto will be formed. Now remove the gold solution; and clean the matrix of its coating of wax. Now put the matrix in voltaic action in a solution of silver, and the face of the new medallion will be filled up with silver. If the body of the medallion is to be silver, the action may be continued for a few days; but if the body is to be of copper, pro-

ceed as before directed with a solution of sulphate of copper. Similar processes give infinite scope to the ingenious in varying and ornamenting this class of voltaic productions.

I am, my dear Sir,

Yours very truly,

W. STURGEON.

To the Rev. J. B. Reade.

XXXVIII. *Contributions to Electricity and Magnetism.*
By JOSEPH HENRY, L.L.D., *Professor of Natural Philosophy, in the College of New Jersey, Princeton.**

On Electro-Dynamic Induction.

INTRODUCTION.

1. Since my investigations in reference to the influence of a spiral conductor, in increasing the intensity of a galvanic current, were submitted to the Society, the valuable paper of Dr. Faraday, on the same subject, has been published, and also various modifications of the principle have been made by Sturgeon, Masson, Page, and others, to increase the effects. The spiral conductor has likewise been applied by Cav. Antinori to produce a spark by the action of a thermo-electrical pile: and Mr. Watkins has succeeded in exhibiting all the phenomena of hydro-electricity by the same means. Although the principle has been much extended by the researches of Dr. Faraday, yet I am happy to state that the results obtained by this distinguished philosopher are not at variance with those given in my paper.

2. I now offer to the Society a new series of investigations in the same line, which I hope may also be considered of sufficient importance to merit a place in the Transactions.

3. The primary object of these investigations was to discover, if possible, inductive actions in common electricity analogous to those found in galvanism. For this purpose a series of experiments was commenced in the spring of 1836, but I was at that time diverted, in part, from the immediate object of my research, by a new investigation of the phenomena known in common electricity by the name of the lateral discharge. Circumstances prevented my doing any thing further, in the way of experiment, until April last, when most of the results which I now offer to the Society were obtained. The investigations are not as complete, in

* Communicated by the Author.

several points, as I could wish, but as my duties will not permit me to resume the subject for some months to come, I therefore present them as they are; knowing, from the interest excited by this branch of science in every part of the world, that the errors which may exist will soon be detected, and the truths be further developed.

4. The experiments are given nearly in the order in which they were made; and in general they are accompanied by the reflections which led to the several steps of the investigation. The whole series is divided, for convenience of arrangement, into six sections, although the subject may be considered as consisting, principally, of two parts. The first relating to a new examination of the induction of galvanic currents; and the second to the discovery of analogous results in the discharge of ordinary electricity.*

5. The principal articles of apparatus used in the experiments, consist of a number of flat coils of copper riband, which will be designated by the names of coil No. 1, coil No. 2, &c.: also of several coils of long wire; and these, to distinguish them from the ribands, will be called helix No. 1, helix No. 2, &c.

6. Coil No. 1 is formed of thirteen pounds of copper plate, one inch and a half wide and ninety-three feet long. It is well covered with two coatings of silk, and was generally used in the form represented in fig. 2, Plate VI., which is that of a flat spiral sixteen inches in diameter. It was however sometimes formed into a ring of larger diameter, as is shown in fig. 5.

7. Coil No. 2 is also formed of copper plate, as the same width and thickness as coil No. 1. It is, however, only sixty feet long. Its form is shown at *b*, fig. 2. The opening at the centre is sufficient to admit helix No. 1. Coils No. 3, 4, 5, 6, &c. are all about sixty feet long, and of copper plate of the same thickness but of half the width of coil, No. 1.

8. Helix No. 1 consists of sixteen hundred and sixty yards of copper wire, $\frac{1}{16}$ th of an inch in diameter. No. 2, of nine hundred and ninety yards: and No. 3, of three hundred and fifty yards, of the same wire. These helices are shown in fig. 3, and are so adjusted in size as to fit into each other; thus forming one long helix of three thousand yards: or, by using them separately, and in different combinations,

* The several paragraphs are numbered in succession, from the first to the last, after the mode adopted by Mr. Faraday, for convenience of reference.

seven helices of different lengths. The wire is covered with cotton thread, saturated with bees' wax, and between each stratum of spires a coating of silk is interposed.

9. Helix No. 4, is shown at *a*, fig. 5; it is formed of five hundred and forty-six yards of wire; $\frac{1}{15}$ th of an inch in diameter, the several spires of which are insulated by a coating of cement. Helix No. 5 consists of fifteen hundred yards of silvered copper wire, $\frac{1}{15}$ th of an inch in diameter, covered with cotton, and is of the form of No. 4.

10. Besides these I was favoured with the loan of a large spool of copper wire, covered with cotton, $\frac{1}{15}$ th of an inch in diameter, and five miles long. It is wound on a small axis of iron, and forms a solid cylinder of wire, eighteen inches long, and thirteen in diameter.

11. For determining the direction of induced currents, a magnetizing spiral was generally used, which consists of about thirty spires of copper wire, in the form of a cylinder, and so small as just to admit a sewing needle into the axis.

12. Also a small horse-shoe is frequently referred to, which is formed of a piece of soft iron, about three inches long, and $\frac{1}{4}$ ths of an inch thick; each leg is surrounded with about five feet of copper bell wire. This length is so small, that only a current of electricity of considerable quantity can develop the magnetism of the iron. The instrument is used for indicating the existence of such a current.

13. The battery used in most of the experiments is shown in fig. 2. It is formed of three concentric cylinders of copper, and two interposed cylinders of zinc. It is about eight inches high, five inches in diameter, and exposes about one square foot and three quarters of zinc surface, estimating both sides of the metal. In some of the experiments a larger battery was used, weakly charged, but all the results mentioned in the paper, except those with a Cruickshank trough, can be obtained with one or two batteries of the above size, particularly if excited by a strong solution. The manner of interrupting the circuit of the conductor by means of a rasp, *b*, is shown in the same figure.

SECTION I.

Conditions which influence the induction of a Current on itself.

14. The phenomenon of the spiral conductor is at present known by the name of the induction of a current on itself, to distinguish it from the induction of the secondary current,

discovered by Dr. Faraday. The two, however, belong to the same class, and experiments render it probable that the spark given by the long conductor is, from the natural electricity of the metal, disturbed for an instant by the induction of the primary current. Before proceeding to the other parts of these investigations, it is important to state the results of a number of preliminary experiments, made to determine more definitely the conditions which influence the action of the spiral conductor.

15. When the electricity is of low intensity, as in the case of the thermo-electrical pile, or a large single battery weakly excited with dilute acid, the flat riband coil No. 1, ninety-three feet long, is found to give the most brilliant deflagrations, and the loudest snaps from a surface of mercury. The shocks, with this arrangement, are, however, very feeble, and can only be felt in the fingers or through the tongue.

16. The induced current in a short coil, which thus produces deflagration, but not shocks, may, for distinction, be called one of quantity.

17. When the length of the coil is increased, the battery continuing the same, the deflagrating power decreases, while the intensity of the shock continually increases. With five riband coils, making an aggregate length of three hundred feet, and the small battery, fig. 2, the deflagration is less than with coil No. 1, but the shocks are more intense.

18. There is, however, a limit to this increase of intensity of the shock, and this takes place when the increased resistance or diminished conduction of the lengthened coil begins to counteract the influence of the increasing length of the current. The following experiment illustrates this fact. A coil of copper wire, $\frac{1}{16}$ th of an inch in diameter, was increased in length by successive additions of about thirty-two feet at a time. After the first two lengths, or sixty-four feet, the brilliancy of the spark began to decline, but the shocks constantly increased in intensity, until a length of five hundred and seventy-five feet was obtained, when the shocks also began to decline. This was then the proper length to produce the maximum effect with a single battery, and a wire of the above diameter.

19. When the intensity of the electricity of the battery is increased, the action of the short riband coil decreases. With a Cruickshank's trough of sixty plates, four inches square, scarcely any peculiar effect can be observed, when the coil forms a part of the circuit. If however the length of the coil be increased in proportion to the intensity of the current, then the inductive influence becomes apparent.

When the current, from ten plates of the above-mentioned trough, was passed through the wire of the large spool (10), the induced shock was too severe to be taken through the body. Again, when a small trough of twenty-five one-inch plates, which alone would give but a very feeble shock, was used with helix No. 1, an intense shock was received from the induction, when the contact was broken. Also a slight shock in this arrangement is given when the contact is formed, but it is very feeble in comparison with the other. The spark, however, with the long wire and compound battery is not as brilliant as with the single battery and the short ribbon coil.

20. When the shock is produced from a long wire, as in the last experiments, the size of the plates of the battery may be very much reduced, without a corresponding reduction of the intensity of the shock. This is shown in an experiment with the large spool of wire (10). A very small compound battery was formed of six pieces of copper bell wire, about one inch and a half long, and an equal number of pieces of zinc of the same size. When the current from this was passed through the five miles of the wire of the spool, the induced shock was given at once to twenty-six persons joining hands. This astonishing effect placed the action of a coil in a striking point of view.

21. With the same spool and the single battery used in the former experiments, no shock, or at most a very feeble one, could be obtained. A current, however, was found to pass through the whole length, by its action on the galvanometer; but it was not sufficiently powerful to induce a current which could counteract the resistance of so long a wire.

22. The induced current in these experiments may be considered as one of *considerable intensity*, and *small quantity*.

23. The form of the coil has considerable influence on the intensity of the action. In the experiments of Dr. Faraday, a long cylindrical coil of thick copper wire, inclosing a rod of soft iron, was used. This form produces the greatest effect when magnetic reaction is employed; but in the case of simple galvanic induction, I have found the form of the coils and helices represented in the figures most effectual. The several spires are more nearly approximated, and therefore they exert a greater mutual influence. In some cases, as will be seen hereafter, the ring form, shown in fig. 5, is most effectual.

24. In all cases the several spires of the coil should be well insulated, for although in magnetizing soft iron, and in

analogous experiments, the touching of two spires is not attended with any great reduction of action; yet in the case of the induced current, as will be shown in the progress of these investigations, a single contact of two spires is sometimes sufficient to neutralize the whole effect.

25. It must be recollected that all the experiments with these coils and helices, unless otherwise mentioned, are made without the reaction of iron temporarily magnetized; since the introduction of this would, in some cases, interfere with the action, and render the results more complex.

SECTION II.

Conditions which influence the production of Secondary Currents.

26. The secondary currents, as it is well known, were discovered in the induction of magnetism and electricity, by Dr. Faraday, in 1831. But he was at that time urged to the exploration of new, and apparently richer veins of science, and left this branch to be traced by others. Since then, however, attention has been almost exclusively directed to one part of the subject, namely, the induction from magnetism, and the perfection of the magneto-electrical machine. And I know of no attempts, except my own, to review and extend the purely electrical part of Dr. Faraday's admirable discovery.

27. The energetic action of the flat coil, in producing the induction of a current on itself, led me to conclude that it would also be the most proper means for the exhibition and study of the phenomena of the secondary galvanic currents.

28. For this purpose coil No. 1 was arranged to receive the current from the small battery, and coil No. 2 placed on this, with a plate of glass interposed to insure perfect insulation; as often as the circuit of No. 1 was interrupted, a powerful secondary current was induced in No. 2. The arrangement is the same as that exhibited in fig. 4, with the exception that in this the compound helix is represented as receiving the induction, instead of coil No. 2.

29. When the ends of the second coil were rubbed together, a spark was produced at the opening. When the same ends were joined by the magnetizing spiral (11), the inclosed needle became strongly magnetic. Also when the secondary current was passed through the wires of the iron horse-shoe (12), magnetism was developed; and when the ends of the second coil were attached to a small decomposing

apparatus, of the kind which accompanies the magneto-electrical machine, a stream of gas was given off at each pole. The shock, however, from this coil is very feeble, and can scarcely be felt above the fingers.

30. This current has therefore the properties of one of moderate intensity, but considerable quantity.

31. Coil No. 1 remaining as before, a longer coil, formed by uniting Nos. 3, 4, and 5, was substituted for No. 2. With this arrangement, the spark produced when the ends were rubbed together, was not as brilliant as before; the magnetizing power was much less; decomposition was nearly the same, but the shocks were more powerful, or, in other words, the intensity of the induced current was increased by an increase of the length of the coil, while the quantity was apparently decreased.

32. A compound helix, formed by uniting Nos. 1 and 2, and therefore containing two thousand six hundred and fifty yards of wire, was next placed on coil No. 1. The weight of this helix happened to be precisely the same as that of coil No. 2, and hence the different effects of the same quantity of metal in the two forms of a long and short conductor, could be compared. With this arrangement the magnetizing effects, with the apparatus before mentioned, disappeared. The sparks were much smaller, and also the decompositions less, than with the short coil; but the shock was almost too intense to be received with impunity, except through the fingers of one hand. A circuit of fifty-six of the students of the senior class, received it at once from a single rupture of the battery current, as if from the discharge of a Leyden jar weakly charged. The secondary current in this case was one of small quantity, but of great intensity.

33. The following experiment is important in establishing the fact of a limit to the increase of the intensity of the shock, as well as the power of decomposition, with a wire of a given diameter. Helix No. 5, which consists of wire only, $\frac{1}{16}$ th of an inch in diameter, was placed on coil No. 2, and its length increased to about seven hundred yards. With this extent of wire, neither decomposition nor magnetism could be obtained, but shocks were given of a peculiarly pungent nature; they did not however produce much muscular action. The wire of the helix was further increased to about fifteen hundred yards; the shock was now found to be scarcely perceptible, in the fingers.

34. As a counterpart to the last experiment, coil No. 1 was formed into a ring of sufficient internal diameter to admit the great spool of wire (11), and with the whole length of

this (which, as has before been stated, is five miles) the shock was found so intense as to be felt at the shoulder, when passed only through the forefinger and thumb. Sparks and decomposition were also produced, and needles rendered magnetic. The wire of this spool is $\frac{1}{8}$ th of an inch thick, and we therefore see from this experiment, that by increasing the diameter of the wire, its length may also be much increased, with an increased effect.

35. The fact (33) that the induced current is diminished by a further increase of the wire, after a certain length has been attained, is important in the construction of the magneto-electrical machine, since the same effect is produced in the induction of magnetism. Dr. Goddard of Philadelphia, to whom I am indebted for coil No. 5, found that when its whole length was wound on the iron of a temporary magnet, no shocks could be obtained. The wire of the machine may therefore be of such a length, relative to its diameter, as to produce shocks, but no decomposition; and if the length be still further increased, the power of giving shocks may also become neutralized.

36. The inductive action of coil No. 1, in the foregoing experiments, is precisely the same as that of a temporary magnet in the case of the magneto-electrical machine. A short thick wire around the armature gives brilliant deflagrations, but a long one produces shocks. This fact, I believe, was first discovered by my friend Mr. Saxton, and afterwards investigated by Sturgeon and Lenz.

37. We might, at first sight, conclude, from the perfect similarity of these effects, that the currents, which, according to the theory of Ampère, exist in the magnet, are like those in the short coil, of great quantity and feeble intensity; but succeeding experiments will show that this is not necessarily the case.

38. All the experiments given in this section have thus far been made with a battery of a single element. This condition was now changed, and a Cruickshank trough of sixty pairs substituted. When the current from this was passed through the riband coil No. 1, no indication, or a very feeble one, was given of a secondary current in any of the coils or helices, arranged as in the preceding experiments. The length of the coil, in this case, was not commensurate with the intensity of the current from the battery. But when the long helix No. 1, was placed instead of coil No. 1, a powerful inductive action was produced on each of the articles, as before.

39. First, helices No. 2 and 3 were united into one, and placed within helix No. 1, which still conducted the battery current. With this disposition a secondary current was produced, which gave intense shocks but feeble decomposition, and no magnetism in the soft iron horseshoe. It was therefore one of intensity, and was induced by a battery current also of intensity.

40. Instead of the helix used in the last experiment for receiving the induction, one of the coils (No. 3) was now placed on helix No. 1, the battery remaining as before. With this arrangement the induced current gave no shocks, but it magnetized the small horseshoe; and when the ends of the coil were rubbed together, produced bright sparks. It had therefore the properties of a current of quantity; and it was produced by the induction of a current, from the battery, of intensity.

41. This experiment was considered of so much importance, that it was varied and repeated many times, but always with the same result; it therefore establishes the fact *that an intensity current can induce one of quantity*, and, by the preceding experiments, the converse has also been shown, *that a quantity current can induce one of intensity*.

42. This fact appears to have an important bearing on the law of the inductive action, and would seem to favour the supposition that the lower coil, in the two experiments with the long and short secondary conductors, exerted the same amount of inductive force, and that in one case this was expended (to use the language of theory) in giving a great velocity to a small quantity of the fluid, and in the other in producing a slower motion in a larger current; but in the two cases, were it not for the increased resistance to conduction in the longer wire, the quantity multiplied by the velocity would be the same. This, however, is as yet a hypothesis, but it enables us to conceive how intensity and quantity may both be produced from the same induction.

43. From some of the foregoing experiments we may conclude, that the quantity of electricity in motion in the helix is really less than in the coil, of the same weight of metal; but this may possibly be owing simply to the greater resistance offered by the longer wire. It would also appear, if the above reasoning be correct, that to produce the most energetic physiological effects, only a small quantity of electricity, moving with great velocity, is necessary.

44. In this and the preceding section, I have attempted to give only the general conditions which influence the galvanic induction. To establish the law would require a great num-

ber of more refined experiments, and the consideration of several circumstances which would affect the results, such as the conduction of the wires, the constant state of the battery, the method of breaking the circuit with perfect regularity, and also more perfect means than we now possess of measuring the amount of the inductive action ; all these circumstances render the problem very complex.

SECTION III.

On the Induction of Secondary Currents at a distance.

45. In the experiments given in the two preceding Sections, the conductor which received the induction, was separated from that which transmitted the primary current by the thickness only of a pane of glass ; but the action from this arrangement was so energetic, that I was naturally led to try the effect at a greater distance.

46. For this purpose coil No. 1 was formed into a ring of about two feet in diameter, and helix No. 4 placed as is shown in fig. 5. When the helix was at the distance of about sixteen inches from the middle of the plane of the ring, shocks could be perceived through the tongue, and these rapidly increased in intensity as the helix was lowered, and when it reached the plane of the ring they were quite severe. The effect, however, was still greater, when the helix was moved from the centre to the inner circumference, as at *c* : but when it was placed without the ring, in contact with the outer circumference, at *b*, the shocks were very slight ; and when placed within, but its axis at right angles to that of the ring, not the least effect could be observed.

47. With a little reflection, it will be evident that this arrangement is not the most favourable for exhibiting the induction at a distance, since the side of the ring, for example, at *c*, tends to produce a current revolving in one direction in the near side of the helix, and another in an opposite direction in the farther side. The resulting effect is therefore only the difference of the two, and in the position as shown in the figure ; this difference must be very small, since the opposite sides of the helix are approximately at the same distance from *c*. But the difference of action on the two sides constantly increases as the helix is brought near the side of the ring, and becomes a maximum when the two are in the position of internal contact. A helix of larger diameter would therefore produce a greater effect.

48. Coil No. 1 remaining as before, helix No. 1, which is nine inches in diameter, was substituted for the small helix

of the last experiment, and with this the effect at a distance was much increased. When coil No. 2 was added to coil No. 1, and the currents from two small batteries sent through these, shocks were distinctly perceptible through the tongue, when the distance of the planes of the coils and the three helices, united as one, was increased to thirty-six inches.

49. The action at a distance was still further increased by coiling the long wire of the large spool into the form of a ring of four feet in diameter, and placing parallel to this another ring, formed of the four ribands of coils No. 1, 2, 3, and 4. When a current from a single battery of thirty-five feet of zinc surface was passed through the riband conductor, shocks through the tongue were felt when the rings were separated to the distance of four feet. As the conductors were approximated, the shocks became more and more severe; and when at the distance of twelve inches, they could not be taken through the body.

50. It may be stated in this connexion, that the galvanic induction of magnetism in soft iron, in reference to distance, is also surprisingly great. A cylinder of soft iron, two inches in diameter and one foot long, placed in the centre of the ring of copper riband, with the battery above mentioned, becomes strongly magnetic.

51. I may perhaps be excused for mentioning in this communication that the induction at a distance affords the means of exhibiting some of the most astonishing experiments, in the line of *physique amusante*, to be found perhaps in the whole course of science. I will mention one which is somewhat connected with the experiments to be described in the next section, and which exhibits the action in a striking manner. This consists in causing the induction to take place through the partition wall of two rooms. For this purpose coil No. 1 is suspended against the wall in one room, while a person in the adjoining one receives the shock, by grasping the handles of the helix, and approaching it to the spot opposite to which the coil is suspended. The effect is as if by magic, without a visible cause. It is best produced through a door, or thin wooden partition.

52. The action at a distance affords a simple method of graduating the intensity of the shock in the case of its application to medical purposes. The helix may be suspended by a string passing over a pulley, and then gradually lowered down towards the plane of the coil, until the shocks are of the required intensity. At the request of a medical friend, I have lately administered the induced current precisely in this way, in a case of paralysis of a part of the nerves of the face.

53. I may also mention that the energetic action of the spiral conductors enables us to imitate, in a very striking manner, the inductive operation of the magneto-electrical machine, by means of an uninterrupted galvanic current. For this purpose it is only necessary to arrange two coils to represent the two poles of a horse-shoe magnet, and to cause two helices to revolve past them in a parallel plane. While a constant current is passing through each coil, in opposite directions, the effect of the rotation of the helices is precisely the same as that of the revolving armature in the machine.

54. A remarkable fact should here be noted in reference to helix No. 4, which is connected with a subsequent part of the investigation. This helix is formed of copper wire, the spires of which are insulated by a coating of cement instead of thread, as in the case of the others. After being used in the above experiments, a small discharge from a Leyden jar was passed through it, and on applying it again to the coil, I was much surprised to find that scarcely any signs of a secondary current could be obtained.

55. The discharge had destroyed the insulation in some part, but this was not sufficient to prevent the magnetizing of a bar of iron introduced into the opening at the centre. The effect appeared to be confined to the inductive action. The same accident had before happened to another coil of nearly the same kind. It was therefore noted as one of some importance. An explanation was afterwards found in a peculiar action of the secondary current.

SECTION IV.

On the effects produced by interposing different Substances between the Conductors.

56. Sir H. Davy found, in magnetizing needles by an electrical discharge, that the effect took place through interposed plates of all substances, conductors and non-conductors.* The experiment which I have given in paragraph 51 would appear to indicate that the inductive action which produces the secondary current might also follow the same law.

57. To test this the compound helix was placed about five inches above coil No. 1, fig. 6, and a plate of sheet iron, about $\frac{1}{16}$ th of an inch thick, interposed. With this arrangement no shocks could be obtained; although, when the plate was withdrawn, they were very intense.

* Philosophical Transactions, 1821.

58. It was at first thought that this effect might be peculiar to the iron, on account of its temporary magnetism ; but this idea was shown to be erroneous by substituting a plate of zinc of about the same size and thickness. With this the screening influence was exhibited as before.

59. After this a variety of substances was interposed in succession, namely, copper, lead, mercury, acid, water, wood, glass, &c. ; and it was found that all the perfect conductors, such as the metals, produced the screening influence ; but non-conductors, as glass, wood, &c., appeared to have no effect whatever.

60. When the helix was separated from the coil by a distance only equal to the thickness of the plate, a slight sensation could be perceived even when the zinc of $\frac{1}{16}$ th of an inch in thickness was interposed. This effect was increased by increasing the quantity of the battery current. If the thickness of the plate was diminished, the induction through it became more intense. Thus a sheet of tinfoil interposed produced no perceptible influence ; also four sheets of the same were attended with the same result. A certain thickness of metal is therefore required to produce the screening effect, and this thickness depends on the quantity of the current from the battery.

61. The idea occurred to me that the screening might, in some way, be connected with an instantaneous current in the plate, similar to that in the induction by magnetic rotation, discovered by M. Arago. The ingenious variation of this principle by Messrs. Babbage and Herschell, furnished me with a simple method of determining this point.

62. A circular plate of lead was interposed, which caused the induction in the helix almost entirely to disappear. A slip of the metal was then cut out in the direction of a radius of the circle, as is shown in fig. 7. With the plate in this condition, no screening was produced ; the shocks were as intense as if the metal were not present.

63. This experiment however is not entirely satisfactory, since the action might have taken place through the opening of the lead ; to obviate this objection, another plate was cut in the same manner, and the two interposed with a glass plate between them, and so arranged that the opening in the one might be covered by the continuous part of the other. Still shocks were obtained with undiminished intensity.

64. But the existence of a current in the interposed conductor was rendered certain by attaching the magnetizing spiral by means of two wires to the edge of the opening in the circular plate, as is shown in fig. 8. By this arrange-

ment the latent current was drawn out, and its direction obtained by the polarity of a needle placed in the spiral at *b*.

65. This current was a secondary one, and its direction, in conformity with the discovery of Dr. Faraday, was found to be the same as that of the primary current.

66. That the screening influence is in some way produced by the neutralizing action of the current thus obtained, will be clear, from the following experiment. The plate of zinc before mentioned, which is nearly twice the diameter of the helix, instead of being placed between the conductors, was put on the top of the helix, and in this position, although the neutralization was not as perfect as before, yet a great reduction was observed in the intensity of the shock.

67. But here a very interesting and puzzling question occurs. How does it happen that two currents, both in the same direction, can neutralize each other? I was at first disposed to consider the phenomenon as a case of real electrical interference, in which the impulses succeed each other by some regular interval. But if this were true the effect should depend on the length and other conditions of the current in the interposed conductor. In order to investigate this, several modifications of the experiments were instituted.

68. First a flat coil (No. 3) was interposed instead of the plates. When the two ends of this were separated, the shocks were received as if the coil were not present; but when the ends were joined, so as to form a perfect metallic circuit, no shocks could be obtained. The neutralization with the coil in this experiment was even more perfect than with the plate.

69. Again, coil No. 2, in the form of a ring, was placed not between the conductors, but around the helix. With this disposition of the apparatus, and the ends of the coil joined, the shocks were scarcely perceptible, but when the ends were separated, the presence of the coil has no effect.

70. Also when helix No. 1 and 2 were together submitted to the influence of coil No. 1, the ends of the one being joined, the other gave no shock.

71. The experiments were further varied by placing helix No. 2 within a hollow cylinder of sheet brass, and this again within coil No. 2 in a manner similar to that shown in fig. 13 which is intended to illustrate another experiment. In this arrangement the neutralizing action was exhibited, as in the case of the plate.

72. A hollow cylinder of iron was next substituted for the one of brass, and with this also no shocks could be obtained.

73. From these experiments it is evident that the neutralization takes place with currents in the interposed or adjoin-

ing conductors of all lengths and intensities, and therefore cannot, as it appears to me, be referred to the interference of two systems of vibrations.

74. This part of the investigation was, for a time, given up almost in despair, and it was not until new light had been obtained from another part of the inquiry, that any further advances could be made towards a solution of the mystery.

75. Before proceeding to the next Section, I may here state that the phenomenon mentioned, paragraph 54, in reference to helix No. 4, is connected with the neutralizing action. The electrical discharge having destroyed the insulation at some point, a part of the spires would thus form a shut circuit, and the induction in this would counteract the action in the other part of the helix; or, in other words, the helix was in the same condition as the two helices mentioned in paragraph 70, when the ends of the wire of one were joined.

76. Also the same principle appears to have an important bearing on the improvement of the magneto-electrical machine: since the plates of metal which sometimes forms the ends of the spool containing the wire, must necessarily diminish the action, and also from experiment of paragraph 72 the armature itself may circulate a closed current which will interfere with the intensity of the induction in the surrounding wire. I am inclined to believe that the increased effect observed by Sturgeon and Bachhoffner, when a bundle of wire is substituted for a solid piece of iron, is at least in part due to the interruption of these currents. I hope to resume this part of the subject, in connexion with several other points, in another communication to the Society.

77. The results given in this Section may, at first sight, be thought at variance with the statements of Sir H. Davy, that needles could be magnetized by an electrical discharge with conductors interposed. But from his method of performing the experiment, it is evident that the plate of metal was placed between a straight conductor and the needle. The arrangement was therefore similar to the interrupted circuit in the experiment with the cut plate (62), which produces no screening effect. Had the plate been curved into the form of a hollow cylinder, with the two ends in contact, and the needle placed within this, the effect would have been otherwise.

SECTION V.

On the Production and Properties of induced Currents of the Third, Fourth, and Fifth order.

78. The fact of the perfect neutralization of the primary current by a secondary, in the interposed conductor, led me

to conclude that if the latter could be drawn out, or separated from the influence of the former, it would itself be capable of producing a new induced current in a third conductor.

79. The arrangement exhibited in fig. 9, furnishes a ready means of testing this. The primary current, as usual, is passed through coil No. 1, while coil No. 2, is placed over this to receive the induction, with its ends joined to those of coil No. 3. By this disposition the secondary current passes through No. 3; and since this is at a distance, and without the influence of the primary, its separate induction will be rendered manifest by the effects on helix No. 1. When the handles *a*, *b*, are grasped a powerful shock is received, proving the induction of a tertiary current.

80. By a similar but more extended arrangement, as shown in fig. 10, shocks were received from currents of a fourth and fifth order; and with a more powerful primary current, and additional coils, a still greater number of successive inductions might be obtained.

81. The induction of currents of different orders, of sufficient intensity to give shocks, could scarcely have been anticipated from our previous knowledge of the subject. The secondary current consists, as it were, of a single wave of the natural electricity of the wire, disturbed but for an instant by the induction of the primary; yet this has the power of inducing another current, but little inferior in energy to itself, and thus produces effects apparently much greater in proportion to the quantity of electricity in motion than the primary current.

82. Some difference may be conceived to exist in the action of the induced currents, and that from the battery, since they are apparently different in nature; the one consisting, as we may suppose, of a single impulse, and the other of a succession of such impulses, or a continuous action. It was therefore important to investigate the properties of these currents, and to compare the results with those before obtained.

83. First, in reference to the intensity, it was found that with the small battery a shock could be given from the current of the third order to twenty-five persons joining hands; also shocks perceptible in the arms were obtained from a current of the fifth order.

84. The action at a distance was also much greater than could have been anticipated. In one experiment shocks from the tertiary current were distinctly felt through the tongue, when helix No. 1, was at the distance of eighteen inches above the coil transmitting the secondary current.

85. The same screening effects were produced by the interposition of plates of metal between the conductors of the different orders, as those which have been described in reference to the primary and secondary currents.

86. Also when the long helix is placed over a secondary current generated in a short coil, and which is therefore, as we have before shown, one of quantity, a tertiary current of intensity is produced.

87. Again, when the intensity current of the last experiment is passed through a second helix, and another coil is placed over this, a quantity current is again produced. Therefore in the case of these currents, as in that of the primary, *a quantity current can be induced from one of intensity, and the converse*. By the arrangement of the apparatus as shown in fig. 10, these different results are exhibited at once. The induction from coil No. 3, to helix No. 1, produces an intensity current, and from the helix No. 2 and 4, a quantity current.

88. If the ends of coil No. 2, as in the arrangement of fig. 9, be united to helix No. 1, instead of coil No. 3, no shocks can be obtained; the quantity current of coil No. 2, appears not to be of sufficient intensity to pass through the wire of the long helix.

89. Also, no shocks can be obtained from the handles attached to helix No. 2, in the arrangement exhibited in fig. 11. In this case the quantity of electricity in the current from the helix appears to be too small to produce any effect, unless its power is multiplied by passing it through a conductor of many spires.

90. The next inquiry was in reference to the direction of these currents, and this appeared important in connexion with the nature of the action. The experiments of Dr. Faraday would render it probable, that at the beginning and ending of the secondary current, its induction on an adjacent wire is in contrary directions, as is shown to be the case in the primary current. But the whole action of a secondary current is so instantaneous, that the inductive effects at the beginning and ending cannot be distinguished from each other, and we can only observe a single impulse, which, however, may be considered as the difference of two impulses in opposite directions.

91. The first experiment happened to be made with a current of the fourth order. The magnetizing spiral (11) was attached to the ends of coil No. 4, fig. 10, and by the polarity of the needle it was found that this current was in

the same direction with the secondary and primary currents.* By a too hasty generalization, I was led to conclude, from this experiment, that the currents of all orders are in the same direction as that of the battery current, and I was the more confirmed in this from the results of my first experiments on the currents of ordinary electricity. The conclusion, however, caused me much useless labour and perplexity, and was afterwards proved to be erroneous.

92. By a careful repetition of the last experiment, in reference to each current, the important fact was discovered, that *there exists an alternation in the direction of the currents of the several orders, commencing with the secondary*. This result was so extraordinary, that it was thought necessary to establish it by a variety of experiments. For this purpose the direction was determined by decomposition, and also by the galvanometer, but the result was still the same; and at this stage of the inquiry I was compelled to the conclusion that the directions of the several currents were as follows:

Primary current,	+
Secondary current,	+
Current of the third order,	—
Current of the fourth order,	+
Current of the fifth order,	—

93. In the first glance at the above table, we are struck with the fact that the law of alternation is complete, except between the primary and secondary currents, and it appeared that this exception might possibly be connected with the induced current which takes place in the first coil itself, and which gives rise to the phenomena of the spiral conductor. If this should be found to be *minus*, we might consider it as existing between the primary and secondary, and the anomaly would thus disappear. Arrangements were therefore made to fully satisfy myself on this point. For this purpose the decomposition of dilute acid and the use of the galvanometer were resorted to, by placing the apparatus between the ends of a cross wire attached to the extremities of the coil, as in the arrangement described by Dr. Faraday (ninth series); but all the results persisted in giving a direction to this current the same as stated by Dr. Faraday, namely, that of the

* It should be recollected that all the inductions which have been mentioned were produced at the moment of breaking the circuit of the battery current. The induction at the formation of the current is too feeble to produce the effects described.

primary current. I was therefore obliged to abandon the supposition that the anomaly in the change of the current is connected with the induction of the battery current on itself.*

94. Whatever may be the nature or causes of these changes in the direction, they offer a ready explanation of the neutralizing action of the plate interposed between two conductors, since a secondary current is induced in the plate; and although the action of this, as has been shown, is in the same direction as the current from the battery, yet it tends to induce a current in the adjacent conducting matter of a contrary direction. The same explanation is also applicable to all the other cases of neutralization, even to those which take place between the conductors of the several orders of currents.

95. The same principle explains some effects noted in reference to the induction of a current on itself. If a flat coil be connected with the battery, of course sparks will be produced by the induction, at each rupture of the circuit. But if in this condition another flat coil, with its ends joined, be placed on the first coil, the intensity of the shock is much diminished, and when the several spires of the two coils are mutually interposed by winding the two ribands together into one coil, the sparks entirely disappear in the coil transmitting the battery current, when the ends of the other are joined. To understand this, it is only necessary to mention that the induced current in the first coil is a true secondary current, and it is therefore neutralized by the action of the secondary in the adjoining conductor; since this tends to produce a current in the opposite direction.

96. It would also appear from the perfect neutralization which ensues in the arrangement of the last paragraph, that the induced current in the adjoining conductor is more powerful than that of the first conductor; and we can easily see how this may be. The two ends of the second coil are joined, and it thus forms a perfect metallic circuit; while the circuit of the other coil may be considered as partially interrupted, since to render the spark visible the electricity must be projected, as it were, through a small distance of air.

97. We would also infer that two contiguous secondary currents produced by the same induction, would partially counteract each other. Moving in the same direction, they would each tend to induce a current in the other of an opposite direction. This is illustrated by the following experiment: helix No. 1 and 2 were placed together, but not

* Our theory, as given in Vol. I. of these Annals, fully explains the whole phenomena. Edit.

united, above coil No. 1, so that they each might receive the induction; the larger was then gradually removed to a greater distance from the coil, until the intensity of the shock from each was about the same. When the ends of the two were united, so that the shock would pass through the body from the two together, the effect was apparently less than with one helix alone. The result, however, was not as satisfactory as in the case of the other experiments; a slight difference in the intensity of two shocks could not be appreciated with perfect certainty.

SECTION VI.

The production of induced Currents of the different Orders from ordinary Electricity.

98. Dr. Faraday, in the ninth series of his researches, remarks that "the effect produced at the commencement and the end of a current (which are separated by an interval of time when that current is supplied from a voltaic apparatus) must occur at the same moment when a common electrical discharge is passed through a long wire. Whether if it happen accurately at the same moment they would entirely neutralize each other, or whether they would not still give some definite peculiarity to the discharge, is a matter remaining to be examined."

99. The discovery of the fact that the secondary current, which exists but for a moment, could induce another current of considerable energy, gave some indication that similar effects might be produced by a discharge of ordinary electricity, provided a sufficiently perfect insulation could be obtained.

100. To test this a hollow glass cylinder, fig. 12, of about six inches in diameter, was prepared with a narrow riband of tinfoil, about thirty feet long, pasted spirally around the outside, and a similar riband of the same length, pasted on the inside; so that the corresponding spires of the two were directly opposite each other. The ends of the inner spiral passed out of the cylinder through a glass tube, to prevent all direct communication between the two. When the ends of the inner riband were joined by the magnetizing spiral (11), containing a needle, and a discharge from a half gallon jar sent through the outer riband, the needle was strongly magnetized in such a manner as to indicate *an induced current through the inner riband in the same direction as that of the current of the jar*. This experiment was repeated many times, and always with the same result.

101. When the ends of one of the ribands were placed very nearly in contact, a small spark was perceived at the opening, the moment the discharge took place through the other riband.

102. When the ends of the same riband were separated to a considerable distance, a larger spark than the last could be drawn from each end by presenting a ball or the knuckle.

103. Also if the ends of the outer riband were united, so as to form a perfect metallic circuit, a spark could be drawn from any point of the same, when a discharge was sent through the inner riband.

104. The sparks in the two last experiments are evidently due to the action known in ordinary electricity by the name of the lateral discharge. To render this clear, it is perhaps necessary to recall the well known fact, that when the knob of a jar is electrified positively, and the outer coating in connexion with the earth, then the jar contains a small excess of positive electricity beyond what is necessary to perfectly neutralize the negative surface. If the knob be put in communication with the earth, the extra quantity, or the free electricity, as it is sometimes called, will be on the negative side. When the discharge took place in the above experiments, the inner riband became for an instant charged with this free electricity, and consequently threw off from the outer riband, by ordinary induction, the sparks described. It therefore became a question of importance to determine, whether the induced current described in paragraph 100 was not also a result of the lateral discharge, instead of being a true case of a secondary current analogous to those produced from galvanism. For this purpose the jar was charged, first with the outer coating in connexion with the earth, and again with the knob in connexion with the same, so that the extra quantity might be in the one case *plus* and in the other *minus*; but the direction of the induced current was not affected by these changes; it was always the same, namely, from the positive to the negative side of the jar.

105. When, however, the quantity of free electricity was increased, by connecting the knob of the jar with a globe about a foot in diameter, the intensity of magnetism appeared to be somewhat diminished, if the extra quantity was on the negative side; and this might be expected, since the free electricity, in its escape to the earth through the riband, in this case would tend to induce a feeble current in the opposite direction to that of the jar.

106. The spark from an insulated conductor may be considered as consisting almost entirely of this free or extra

electricity, and it was found that this was also capable of producing an induced current, precisely the same as that from the jar. In the experiment which gave this result, one end of the outer riband of the cylinder (100) was connected with the earth, and the other caused to receive a spark from a conductor fourteen feet long, and nearly a foot in diameter. The direction of the induced current was the same as that of the spark from the conductor.

107. From these experiments it appears evident that the discharge from the Leyden jar possesses the property of inducing a secondary current precisely the same as the galvanic apparatus, and also that this induction is only so far connected with the phenomenon of the lateral discharge as this latter partakes of the nature of an ordinary electrical current.

108. Experiments were next made in reference to the production of currents of the different orders by ordinary electricity. For this purpose a second cylinder was prepared with ribands of tinfoil, in a similar manner to the one before described. The two were then so connected that the secondary current from the first would circulate around the second. When a discharge was passed through the outer riband of the first cylinder, a tertiary current was induced in the inner riband of the second. This was rendered manifest by the magnetizing of a needle in a spiral joining the ends of the last mentioned riband.

109. Also by the addition, in the same way, of a third cylinder, a current of the fourth order was developed. The same result was likewise obtained by using the arrangement of the coils and helices shown in fig. 10. For these experiments, however, the coils were furnished with a double coating of silk, and the contiguous conductors separated by a large plate of glass.

110. Screening effects precisely the same as those exhibited in the action of galvanism were produced by interposing a plate of metal between the conductors of different orders, figs. 9 and 10. The precaution was taken to place the plate between two frames of glass, in order to be assured that the effect was not due to a want of perfect insulation.

111. Also analogous results were found when the experiments were made with coils interposed instead of plates, as described in paragraph 68. When the ends of the interposed coils were separated, no screening was observed, but when joined, the effect was produced. The existence of the induced current, in all these experiments, was determined by the magnetism of a needle in a spiral attached to one of the coils.

112. Likewise shocks were obtained from the secondary current by an arrangement shown in fig. 13. Helices No. 2 and No. 3 united are put within a glass jar, and coil No. 2 is placed around the same. When the handles are grasped, a shock is felt at the moment of the discharge, through the outer coil. The shocks, however, were very different in intensity with different discharges from the jar. In some cases no shock was received, when again with a less charge, a severe one was obtained. But there irregularities find an explanation in a subsequent part of the investigation.

113. In all these experiments, the results with ordinary and galvanic electricity are similar. But at this stage of the investigation there appeared what at first was considered a remarkable difference in the action of the two. I allude to the direction of the currents of the different orders. These, in the experiments with the glass cylinders, instead of exhibiting the alternations of the galvanic currents (92), were all in the same direction as the discharge from the jar, or, in other words, they were all *plus*.

114. To discover, if possible, the cause of this difference, a series of experiments was instituted; but the first fact developed, instead of affording any new light, seemed to render the obscurity more profound. When the directions of the currents were taken in the arrangement of the coils (fig. 10) the discrepancy vanished. *Alternations were found the same as in the case of galvanism*. This result was so extraordinary that the experiments were many times repeated, first with the glass cylinders, and then with the coils; the results, however, were always the same. The cylinders gave currents all in one direction; the coils in alternate directions.

115. After various hypotheses had been formed, and in succession disproved by experiment, the idea occurred to me that the direction of the currents might depend on the distance of the conductors, and this appeared to be the only difference existing in the arrangement of the experiments with the coils and the cylinders.* In the former the distance between the ribands was nearly one inch and a half, while in the latter it was only the thickness of the glass, or about $\frac{1}{16}$ th of an inch.

116. In order to test this idea, two narrow slips of tinfoil, about twelve feet long, were stretched parallel to each other, and separated by thin plates of mica to the distance of about

* This idea was not immediately adopted, because I had previously experimented on the direction of the secondary current from galvanism, and found no change in reference to distance.

$\frac{1}{8}$ th of an inch. When a discharge from the half gallon jar was passed through one of these, an induced current in the same direction was obtained from the other. The ribands were then separated, by plates of glass, to the distance of $\frac{1}{4}$ th of an inch; the current was still in the same direction, or *plus*. When the distance was increased to about $\frac{1}{2}$ th of an inch, no induced current could be obtained; and when they were still further separated the current again appeared, but was now *found to have a different direction, or to be minus*. No other change was observed in the direction of the current; the intensity of the induction decreased as the ribands were separated. The existence and direction of the current, in this experiment, were determined by the polarity of the needle in the spiral attached to the ends of one of the ribands.

117. The question at this time arose, whether the direction of the current, as indicated by the polarity of the needle, was the true one, since the magnetizing spiral might itself, in some cases, induce an opposite current. To satisfy myself on this point a series of charges, of various intensity and quantity, from a single spark of the large conductor to the full charge of nine jars, were passed through the small spiral, which had been used in all the experiments, but they all gave the same polarity. The interior of this spiral is so small, that the needle is throughout in contact with the wire.

118. The fact of a change in the direction of the induced current by a change in the distance of the conductors, being thus established, a great number and variety of experiments were made to determine the other conditions on which the change depends. These were sought for in a variation of the intensity and quantity of the primary discharge, in the length and thickness of the wire, and in the form of the circuit. The results were, however, in many cases, anomalous, and are not sufficiently definite to be placed in detail before the Society. I hope to resume the investigation at another time, and will therefore at present briefly state only those general facts which appear well established.

119. With a single half gallon jar, and the conductors separated to a distance less than $\frac{1}{4}$ th of an inch, the induced current is always in the same direction as the primary. But when the conductors are gradually separated, there is always found a distance at which the current begins to change its direction. This distance depends certainly on the amount of the discharge, and probably on the intensity; and also on the length and thickness of the conductors. With a battery of eight half gallon jars, and parallel wires of about ten feet

long, the change in the direction did not take place at a less distance than from twelve to fifteen inches, and with a still larger battery and longer conductors, no change was found, although the induction was produced at the distance of several feet.

120. The facts given in the last paragraph, relate to the inductive action of the primary current; but it appears from the results detailed in paragraphs 110 and 114, that the currents of all the other orders also change the direction of the inductive influence with a change of the distance. In these cases however, the change always takes place at a very small distance from the conducting wire; and in this respect the result is similar to the effect of a *primary current* from the discharge of a small jar.

121. The most important experiments, in reference to distance, were made in the lecture room of my respected friend, Dr. Hare, of Philadelphia, with the splendid electrical apparatus described in the Fifth volume (new series) of the Transactions of this Society. The battery consists of thirty-two jars, each of the capacity of a gallon. A thick copper wire of about $\frac{1}{16}$ th of an inch in diameter and eighty feet in length, was stretched across the lecture room, and its ends brought to the battery, so as to form a trapezium, the longer side of which was about thirty-five feet. Along this side a wire was stretched of the ordinary bell size, and the extreme ends of this joined by a spiral, similar to the arrangement shown in fig. 14. The two wires were at first placed within the distance of about an inch, and afterwards constantly separated after each discharge of the whole battery through the thick wire. When a break was made in the second wire at *a*, no magnetism was developed in a needle in the spiral at *b*, but when the circuit was complete, the needle at each discharge indicated a current in the same direction as that of the battery. When the distance of the two wires was increased to sixteen inches, and the ends of the second wire placed in two glasses of mercury, and a finger of each hand plunged into the metal, a shock was received. The direction of the current was still the same, but the magnetism not as strong as at a less distance.

122. The second wire was next arranged around the other, so as to enclose it. The magnetism by this arrangement appeared stronger than with the last; the direction of the current was still the same, and continued thus, until the two wires were at every point separated to the distance of twelve feet, except in one place where they were obliged to be crossed at the distance of seven feet, but here the wires were

made to form a right angle with each other, and the effect of the approximation was therefore (46) considered as nothing. The needle at this surprising distance was tolerably strongly magnetized, as was shown by the quantity of filings which would adhere to it. The direction of the current was still the same as that of the battery. The form of the room did not permit the two wires to be separated to a greater distance. The whole length of the circuit of the interior large wire was about eighty feet; that of the exterior one hundred and twenty. The two were not in the same plane, and a part of the outer passed through a small adjoining room.

123. The results exhibited in this experiment are such as could scarcely have been anticipated by our previous knowledge of the electrical discharge. They evince a remarkable inductive energy, which has not before been distinctly recognized, but which must perform an important part in the discharge of electricity from the clouds. Some effects which have been observed during thunder storms, appear to be due to an action of this kind.

124. Since a discharge of ordinary electricity produces a secondary current in an adjoining wire, it should also produce an analagous effect in its own wire; and to this cause may be now referred the peculiar action of a long conductor. It is well known that the spark from a very long wire, although quite short, is remarkably pungent. I was so fortunate as to witness a very interesting exhibition of this action during some experiments on atmospheric electricity made by a committee of the Franklin Institute, in 1836. Two kites were attached one above the other, and raised with a small iron wire in place of a string. On the occasion at which I was present, the wire was extended by the kites to the length of about one mile. The day was perfectly clear, yet the sparks from the wire had so much projectile force (to use a convenient expression of Dr. Hare) that fifteen persons joining hands and standing on the ground, received the shock at once, when the first person of the series touched the wire. A Leyden jar being grasped in the hand by the outer coating, and the knob presented to the wire, a severe shock was received, as if by a perforation of the glass, but which was found to be the result of the sudden and intense induction.

125. These effects were evidently not due to the accumulated intensity at the extremities of the wire, on the principles of ordinary electrical distribution, since the knuckle required to be brought within about a quarter of an inch before the spark could be received. It was not alone the quantity, since the experiments of Wilson prove that the same effect is not produced with an equal amount of electricity on the surface

of a large conductor. It appears evidently therefore a case of the induction of an electrical current on itself. The wire is charged with a considerable quantity of feeble electricity, which passes off in the form of a current along its whole length, and thus the induction takes place at the end of the discharge, as in the case of a long wire transmitting a current of galvanism.

126. It is well known that the discharge from an electrical battery possesses great divergent powers; that it entirely separates, in many instances, the particles of the body through which it passes. This force acts, in part, at least, in the direction of the line of the discharge, and appears to be analogous to the repulsive action discovered by Ampère, in the consecutive parts of the same galvanic current. To illustrate this, paste on a piece of glass a narrow slip of tinfoil, cut it through at several points, and loosen the ends from the glass at the places so cut. Pass a discharge through the tinfoil from about nine half gallon jars; the ends, at each separation, will be thrown up, and sometimes bent entirely back, as if by the action of a strong repulsive force between them. This will be understood by a reference to fig. 14; the ends are shown bent back at *a, a, a, a*. In the popular experiment of the pierced card, the bur on each side appears to be due to an action of the same kind.*

127. It now appears probable, from the facts given in paragraphs 119 and 120, that the table in paragraph 92 is only an approximation to the truth, and that each current from galvanism, as well as from electricity, first produces an inductive action in the direction of itself, and that the inverse influence takes place at a little distance from the wire.

128. To test this the compound helix was placed on coil No. 1, to receive the induction, and its ends joined to those of the outer riband of tinfoil of the glass cylinder, while the magnetizing spiral was attached to the ends of the inner riband. A feeble tertiary current was produced by this arrangement, which in two cases gave a polarity to the needle indicating a direction the same as that of the primary current. In other cases the magnetism was either imperceptible or *minus*. With an arrangement of two coils of wires around two glass cylinders, one within the other, the same effect was produced. The magnetism was less when the distance of the two sets of spires was smaller, indicating, as it would

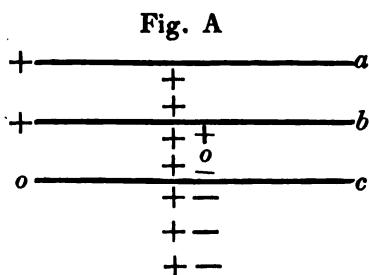
* We have witnessed this fact many years since in our strips of tin foil for protecting jars; Vol. II. p. 86. Even when these strips are laid over the top of the lining, more than an inch, they are sometimes blown off, and much perforated. Edit.

appear, an approximation to a position of neutrality. These results are rather of a negative kind, yet they appear to indicate the same change with distance in the case of the galvanic currents, as in that of the discharge of ordinary electricity. The distance however at which the change takes place would seem to be less in the former than in the latter.

129. There is a perfect analogy between the inductive action of the primary current from the galvanic apparatus and of that from the larger electrical battery. The point of change, in each, appears to be at a great distance.

130. The neutralizing effect described in Section IV. may now be more definitely explained by saying that when a third conductor is acted on at the same time by a primary and secondary current (unless it be very near the second wire) it will fall into the region of the *plus* influence of the former, and into that of the *minus* influence of the latter; and hence no induction will be produced.

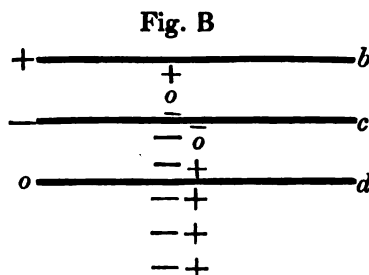
131. This will be rendered perfectly clear by fig. A, in



which *a* represents the conductor of the primary current, *b* that of the secondary, and *c* the third conductor. The characters + + +, &c. beginning at the middle of the first conductor and extending downwards, represent the constant *plus* influence of the primary current, and those + 0 —,

&c., beginning at the second conductor, indicate its inductive influence as changing with the distance. The third conductor, as is shown by the figure, falls in the *plus* region of the primary current, and in the *minus* region of the secondary, and hence the two actions neutralize each other, and no apparent result is produced.

132. Fig. B indicates the method in which the neutralizing



effect is produced in the case of the secondary and tertiary currents. The wire conducting the secondary current is represented by *b*, that conducting the tertiary by *c*, and the other wire, to receive the induction from these, by *d*. The direction of the influence, as before, is indicated by + 0 —,

&c., and the third wire is again seen to be in the *plus* region of the one current, and in the *minus* of the other. If, however, *d* is placed sufficiently near *c*, then neutralization will not take place, but the two currents will conspire to produce in it an induction in the same direction. A similar effect would also be produced were the wire *c*, in fig. A, placed sufficiently near the conductor *b*.

133. Currents of the several orders were likewise produced from the excitation of the magneto-electrical machine. The same neutralizing effects were observed between these as in the case of the currents from the galvanic battery, and hence we may infer that also the same alternations take place in the direction of the several currents.

134. In conclusion, I may perhaps be allowed to state, that the facts here presented have been deduced from a laborious series of experiments, and are considered as forming some addition to our knowledge of electricity, independently of any theoretical considerations. They appear to be intimately connected with various phenomena, which have been known for some years, but which have not been referred to any general law of action. Of this class are the discoveries of Savary, on the alternate magnetism of steel needles, placed at different distances from the line of a discharge of ordinary electricity,* and also the magnetic, screening influence of all metals, discovered by Dr. Snow Harris, of Plymouth.† A comparative study of the phenomena observed by these distinguished *savants*, and those given in this paper, would probably lead to some new and important developments. Indeed every part of the subject of electro-dynamic induction appears to open a field for discovery, which experimental industry cannot fail to cultivate with immediate success.

NOTE.

On the evening of the meeting at which my investigations were presented to the Society, my friend, Dr. Bache of the Girard College, gave an account of the investigations of Professor Ettingshausen, of Vienna, in reference to the improvement of the magneto-electric machine, some of the results of which he had witnessed at the University of Vienna about a year since. No published account of these experiments has yet reached this country, but it appears that Professor Ettingshausen had been led to suspect the develop-

* *Annales de Chimie et de Physique*, 1827.

† *Philosophical Transactions*, 1831.

ment of a current in the metal of the keeper of the magneto-electric machine, which diminished the effect of the current in the coil about the keeper, and hence to separate the coil from the keeper by a ring of wood of some thickness, and afterwards, to prevent entirely the circulation of currents in the keeper, by dividing it into segments, and separating them by a non-conducting material. I am not aware of the result of this last device, nor whether the mechanical difficulties in its execution were fully overcome. It gives me pleasure to learn that the improvements, which I have merely suggested as deductions from the principles of the interference of induced currents (76), should be in accordance with the experimental conclusions of the above named philosopher.

XXXIX. *On Lightning Conductors, and on certain Principles in Electrical Science; being an investigation of Mr. Sturgeon's Experimental and Theoretical Researches in Electricity, published by him in the "Annals of Electricity," &c. By W. SNOW HARRIS, Esq., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the *Annals of Electricity* for October last will be found a memoir on Marine Lightning conductors. This memoir is addressed to the British Association, and is considered by Mr. Sturgeon, the author of it, to merit in a high degree the especial consideration of all the learned scientific bodies in Europe and America.

The author endeavours to show, that a metallic rod whilst transmitting a charge of electricity, is always productive of powerful lateral explosions, not only on near bodies, but on bodies at very great distances. This effect, he thinks, in the case of a lightning rod, is a very fearful circumstance.

2. If this deduction be worth anything, it is altogether subversive of the use of such rods as a means of protection from lightning. I have thought it right, therefore, to examine carefully the experiments and reasonings, which have led the author to this conclusion; and since the inquiry bears materially on a question of great public interest, and contains many new phenomena of electrical action, I hope it may not be considered unworthy a place in your very valuable Journal.

3. Although Mr. Sturgeon has spoken in a slighting way of me and my experiments, and has laboured hard to invalidate them, I still feel, that any personal consideration is comparatively of minor consequence. I will not, therefore, trouble your readers on the subject. I merely wish to have it understood, that this is not a reply to that large part of the memoir levelled at myself, but is simply an investigation of the author's "Theoretical and Experimental Researches," and of his claims to our confidence as a writer on Electrical Science.

4. So long since as the years 1728 and 1729, Mr. Grey observed the phenomena of electrical conduction and insulation.

(a). Thus a metallic ball, J, fig. 1, Plate VII. supported on the glass rod *g*, is said to be insulated, and if electrified, will cause a spark in the opening between the metallic body B and the ball J.

(b). If we connect the ball J with any distant body *c*, by means of a metallic wire as in fig. 2, and electrify it as before, the spark will still occur in the opening at the distant body *c*, the electricity being conducted by the intermediate wire.

(c). The distance at which this effect may ensue, is very considerable. Mr. Grey succeeded in making it sensible at a distance of 765 feet.*

(d). The effect is more sensible when the body B is connected with the ground, which places it, by a law of electrical action, in the most favourable state for receiving the spark.

5. I am desirous to call especial attention to these results, notwithstanding their elementary character, because, as we shall presently see, they are really nothing more or less than the essence of Mr. Sturgeon's *new researches*, and which he claims to have considered by all the learned societies of Europe and America.

6. When we attempt to charge an electrical jar, J. fig. 3, it is observable, that as the charge accumulates on the inner surface, a corresponding quantity of electricity is forced off from the outer, and without this double effect takes place we fail to accumulate a charge.

(e). To render this evident, we have only to place the jar on an insulator, as in fig. 3; we shall then find, that for every spark we send into the jar, a similar spark will leave its outside, either from the coating directly, or from any distant body *c* connected with it as in fig. 4.

* Priestley's History of Electricity.

The outer coating J, therefore, and distant body *c*, may be considered in their insulated state as being insulated conductors under the conditions represented in fig. 2.

(*f*). Suppose the jar charged, and that it remains insulated; then we may discharge it, either by one dense shock through the rod *t*, fig. 4, or gradually, in the reverse way of charging; viz. by continuing to draw sparks from the knob *m*, and add them to the coating J: the circumstance however of our being enabled to take a finite spark, from either side alternately, whilst the jar rests on an insulator, is sufficient to show, that the accumulated electricity is never exactly balanced between the opposed coatings, so that there will always be an excess of either positive or negative electricity over the neutralizing quantities themselves, disposed on the coatings of the jar.

(*g*). When therefore we discharge the jar, this excess of free electricity will speedily expand itself over the outer surface J, the discharging rod *t*, the knob of the jar *m*, or any other body, *c*, fig. 4, connected with it, which, as in the case of the simply electrified conductor, J, fig. 2, will cause a spark to occur in either of those places. The intensity of this spark however will depend on the capacity of the jar. It is *less* with a large jar, and *greater* with a small one, the quantity of electricity discharged being the same.

(*h*). When the jar has been discharged, the knob, the outer coating, and all the bodies connected with it, will be found in the same electrical state. We may make this state either positive or negative, by taking a spark either from the knob or coating previously to discharging the jar.

(*i*). This small spark caused by the excess of free electricity, may be obtained even though the jar be connected with the earth, provided we seize it before the conductors have had time to operate in carrying off the residuary accumulation; Professor Wheatstone having shown by his unrivalled experiments on electrical conduction, that some portion of time elapses in the passage of electricity through wires.

By bringing a metallic ball, B, fig. 3 and 4, therefore in a free state, either very near the discharging rod *c*, fig. 3, the outer coating J, or any body, *c*, fig. 4, in connexion with it, previously to making the discharge, we seize as it were some of the residuary electricity before it has time to pass off, and hence it becomes evident in this particular direction. The effect, however, will be necessarily greatest when the jar and its appendages are quite insulated. After this spark has taken place, the jar will be found again slightly charged, with what has been called a residuary charge, so that the phenomenon

itself is actually the *same as that already* observed in charging the jar originally (*e*).

7. Now these simple experiments (*g*), (*h*), (*i*), are just the experiments described by Mr. Sturgeon, in which he imagines that the small spark above described, is produced by a lateral action of the rod carrying off the discharge. He seems to consider it as a novel and important fact, and calls upon the "principal scientific bodies in Europe and America," and "the ablest electricians the world can produce," in order that it may be fully sifted and explained. He takes great credit for having placed this subject before them in a "*proper light*," and cannot account for the circumstance of my having overlooked it.*

8. But since it is clear that this supposed lateral explosion really resolves itself into one or two simple facts (*a*) (*b*), known to electricians for more than a century since, "the ablest electricians the world can produce," may, perhaps, be disposed to think such an occupation of their time unnecessary, and the several "Learned Societies in Europe and America" may consider it would have been quite as well for Mr. Sturgeon's credit, as a lecturer on natural philosophy, if he had not troubled them on the occasion.

9. The following is Mr. Sturgeon's version of these experiments :

This kind of lateral discharge, "consists in the displacement of the electrical fluid of bodies vicinal to a continuous conductor carrying the primitive discharge."

Exp.—If a Leyden jar, *J*, fig 2, be discharged through a rod *c c*, a spark will appear at the opening *o*, between the metallic body *B* placed near the rod.

Exp.—If instead of discharging the jar through the rod *c c*, fig. 4, we discharge it by a common discharging rod *t*, still the spark will appear at *o*, as before.

"The effect," he says, "is much increased by connecting the body *B* with the ground, and diminished to a certain extent by connecting the outside of the jar with the ground." I have produced the spark, he says, between *c c*, and the body *B* when placed at 50 feet from the *direct* discharge.

"By this kind of lateral discharge," he observes, "a dense spark may be produced when the bodies *B* and *c c*, fig. 3, are half an inch apart. Though the jar be only of the capacity of a quart, chemical decompositions may be effected by it."

* "I mean to submit the substance of my Memoir to the consideration of the principal scientific bodies in Europe and America, in order that the subject may be fully sifted and explained by the ablest electricians the world can produce."—*Ann. of Elect.*, p. 191.

10. Mr. Sturgeon does not state precisely how these experiments were conducted, but the nature of the manipulations would have a material effect on the result. If for example a small jar of a quart capacity were charging from a very powerful machine, and the discharge produced at the time of charging, either by a spontaneous explosion between the balls, *mc*, fig. 3, or by an insulated discharger, then, as is evident, not only would the outer coating and its appendages become charged with the residuary electricity proper to the jar, but also by electricity from the prime conductor, which would assuredly pass over at the instant of the discharge. In Mr. Sturgeon's account of his experiments this fallacious method would appear to have been resorted to. He says, "a spark is felt at every discharge through the circuit represented in the figure," that is *mc*, fig. 3. Now the continued discharges implied in this statement, could only be produced by continuing to work the machine in connexion with the jar. This circumstance alone would be sufficient to falsify the whole.

21. The following experiments are not unimportant as bearing on the present question.

(*k*). Let a jar *J*, fig. 3, be charged positively, removed from the machine, and insulated.—Under this condition discharge it. When discharged, let the electrical state of the knob *m*, discharging conductor *cc*, the outer coating *J*, or any distant body *cc*, fig. 4, connected with it, be examined; they will all be found in the same electrical state, which state will be precisely that, exhibited by the outer coating and knob, whilst charging; and the small residuary spark will be plus.

(*l*). Charge the jar as before; but before discharging it, withdraw the free electricity from the knob. The electrical state of the coating and appendages will be now changed, and the small residuary spark will be minus.

(*m*). Immediately after the discharge, apply a metallic body *B*, fig. 3 and 4, either to the coating *J*, or any body connected with it. A residuary spark will be thrown off.

(*n*). Place a metallic body *B* near the discharger, or outer coating, previously to making the discharge; the spark will then appear to ensue at the time of the discharge.

(*o*). Examine the jar after this residuary spark has been taken from the outer coating, and it will be found again slightly charged as at first.

(*p*). Charge a jar, exposing about two square feet of coating, with a given quantity of electricity, measured by the unit jar *u*, fig. 5. Let a conducting rod terminating in a ball *r*, project from the outer coating, and place near it the electro-

scope E.* Discharge the jar through the rod *c c*, as before, and observe the amount of divergence of the electroscope. Double the capacity of the jar, and again accumulate and discharge the same quantity. The divergence of the electroscope will be very considerably decreased. Add a second and a third jar to the former, and the effect will be at last scarcely perceptible: connect the jar with the ground, and with a given quantity the spark will vanish altogether.

(*q*). Accumulate a given quantity as before, and observe the effect of the residuary charge on the electroscope. Let a double, treble, &c., quantity be accumulated and discharged from a double, treble, &c., extent of surface; that is to say, for a double quantity employ two similar jars, and so on: the effect will remain the same.

(*r*). The quantity and surface remaining constant, let the discharge be effected by discharging circuits *c c*, fig 3, of different dimensions from a large rod down to a fine wire which the charge in passing can make red-hot. Observe the effect on the electroscope in each case: it will be found nearly the same, being rather less where the tension in the discharging wire is very considerable.

(*s*). Connect the jar with the ground, and place between the discharging conductor *c* fig. 3, and a metallic mass B, a small quantity of percussion powder, inclosed in thin paper. The powder will not be inflamed, even in the case of the discharging conductor becoming red-hot: whereas in passing the slightest spark, it inflames directly.

(*t*). Insulate a circular conducting disc, M, fig. 6, of four feet in diameter: it may be made of wood covered with tin foil; oppose to it a similar disc, N, connected with the ground. Place a conducting rod, *c c*, on the lower plate, and near it a metallic body, *o*; electrify the upper plate, *m*; dense sparks will fall on the rod, *c c*, but no effect is observable on the vicinal body, *o*, even though percussion powder be placed in the opening.

12. These experiments are conclusive of the nature of Mr. Sturgeon's experiments.

Exp. (*k*). (*l*).—show, that the electricity of the spark varies with that of the coatings.

Exp. (*m*).—proves that the spark is readily obtained *after* the discharge has taken place; it is not therefore any lateral explosion caused by the discharging rod.

* The electroscope I employed is described in the Transactions of the Royal Society for 1834, Part 2, page 214. For more accurate measurement we should employ the electrometer, p. 215.

Exp. (o).—proves that the spark is merely a residual accumulation.

Exp. (p). (q).—prove that the spark is of different degrees of force, when the electricity is discharged from a greater or less extent of surface, whilst double, treble, &c., quantities, when discharged from double, treble, &c., surfaces, give the same spark. Now as no one can doubt but that the effect of a double, &c. quantity should be greater than a single, &c. quantity, it is again evident that the spark is not caused by any lateral explosion from the discharging rod; it being a well-established law, that the same quantity has the same heating effect on wires, whether discharged from a great surface or a small one, from thick glass or thin; some little allowance being made for the greater number of rods, &c., when the surface is increased by an additional number of jars.* The effect therefore depending on the jar, Mr. Sturgeon had a greater chance with a small jar than with a large one.

Exp. (r).—proves that the degree of tension in the rod is not of any consequence.

Exp. (s). (t).—show, that no kind of lateral action arises during the passage of the charge.

13. Mr. Sturgeon confounds this residuary spark, with the Earl of Stanhope's experiments on induction: he observes, p. 176, "Viscount Mahon studied this kind of lateral discharge very extensively." But any one who considers His Lordship's work, will soon detect the fallacy of such a conclusion. Lord Mahon shows, that when an electrical charge is about to pass from a body M, fig 7, in the direction C L, the action upon a near body N will displace some of its electricity; hence a spark will take place at E between that body and another connected with the ground whenever the discharge takes place from M, in consequence of the return of the displaced electricity. This effect His Lordship termed the "returning stroke." Now to apply this to the operation of a thunder cloud. Let M, fig. 6, represent a mass of cloud covering a portion of the earth's surface N. Let *c c* be a discharging rod, and *o* some near body. Then by Lord Stanhope's experiment the charged cloud M will displace from the surface N, and all the bodies on it as *c c*, *o*, &c. a portion of their natural electricity, which will again return when the discharge has been effected. The conditions of Lord Mahon's experiment cannot obtain between the conductor *c c* and the

* Philosophical Transactions for 1834. Part II. p. 225, and Faraday's Researches.

body *o*, since they are both in the same forced state.* It is very easy to perceive, that the electrical relations of two bodies *o* and *c* *between* the boards, is different from that between a conductor J, fig. 1, charged with electricity, and a body B in its natural state; or that of a conductor C, fig. 6, carrying off the displaced electricity of the lower plate N, and a body B. neutral. Besides, in Lord Mahon's experiment, fig. 7, the electricity of the return spark is different from that of the primitive charge in M; whereas, in Mr. Sturgeon's experiment, the spark is of the same kind. So little did His Lordship anticipate any objection to the use of lightning rods in consequence of his experiments, that he declares his conviction of their passive operation, and reproves those who "ignorantly conclude" that they are of a dangerous nature.

14. We have been here discussing what the author calls a *third* kind of lateral discharge; but he mentions a *first* and *second* kind also. The first kind, he says, "takes place at every interruption of a metallic circuit;" "it displaces loose bodies," &c. This is evidently the effect of mechanical expansion, and is the very effect we avoid by means of a lightning rod. He alludes to Dr. Priestley as authority on this point; how unfortunate for his whole doctrine! Let us consider for a moment what Dr. Priestley says: "That the cause of this dispersion of bodies in the neighbourhood of electrical explosions is *not their being suddenly charged with electric matter*, is, I think, evident. I never observed the *least attraction of these bodies toward the brass rods, through which the explosion passed*, although I used several methods which could not fail to show it. I even found that the explosion of a battery made ever so near a brass rod, did not so much as disturb its electric fluid; for when I had insulated the rod, and hung a pair of pith balls on the end opposite to that near which the explosion passed, I found the balls were not in the least moved.†

* This applies to Mr. Sturgeon's Exp. (9).—If B fig. c, 3, were on the same insulation with the jar J and rod *c*, *no spark could occur at o*, except by a division of the charge, whatever quantity passed through *c*. This fact alone is conclusive of the point in question, proving clearly that the spark is *not* a lateral explosion.

† The reader will distinguish here between this experiment and Lord Mahon's. The latter relates to the influence of a permanently charged conductor on a body neutral; whereas Priestley's applies to the action of wires carrying vanishing quantities of electricity, the very essence of Mr. Sturgeon's experiment. Dr. Priestley would not have told us, had he brought his rod near the *free side* of his battery, that then the pith balls were not moved.

We have seen how little support Mr. Sturgeon derived from Lord Mahon; he obtains still less from Priestley, who, without any compromise, sweeps away his whole theory. Lord Stanhope and Dr. Priestley, eminent amongst the philosophers of their day, will be doubtless admitted to be as good authority as Mr. Sturgeon.

15. The *second* kind of lateral discharge is, we are informed, "a radiation of electric matter from conductors carrying the primitive discharge." It takes place, the author says, from edges, and that hence "sharp edges of metal carrying a flash of lightning would discharge necessarily a great quantity of fluid into neighbouring bodies." No author is pressed into the service on this occasion, and for the best possible reason, no accredited writer has ever treated of such a phenomenon as applying to a lightning rod. It is in fact applicable only to charged conductors. Thus ragged or pointed rods attached to the prime conductor of the electrical machine exhibit brushes of light, whilst other similar bodies, within their influence, have the appearance of stars. The lights on steeples, and on the sail yard and masts of ships, mentioned by Pliny, are of this kind. Franklin explained these phenomena, and showed that pointed bodies were favourable to the rapid dissipation of electrical accumulations, and, as is well known, availed himself of the important fact in his application of the pointed lightning rod. How Mr. Sturgeon has contrived to associate this effect with the effects of discharges of lightning *through* conductors it is difficult to say. It is certainly a very strange confusion of things. That the effect in question has nothing to do with a sharp or round edge, or angular discharges, may be shown by the following experiments:—

(u). Dr. Priestley discharged a battery over a wire circuit perfectly straight, and also over the same circuit passed about pins so as to make sharp angles:—the result of the charge on fusing a given length of wire was not influenced, which could hardly have been if the angular portion had thrown off or discharged into the neighbouring pins, &c. any of the charge, it being well known that the least diminution of quantity is fatal to a delicate experiment on the fusion of wire.

(v). Discharge a given quantity of electricity by a continuous rod free of edges, through a wire passed through the ball of an air thermometer, and also by a similar rod with ragged edges, placed near other metallic masses: the effect on the wire remains unchanged.*

* For a description of this instrument, termed an electro-thermometer, see Transactions of the Royal Society for 1827, p. 18.

It is not difficult to perceive the distinction of the two cases just alluded to. If Dr. Priestley had *insulated* his wire, and then charged it in the ordinary way, brushes of light would doubtless have escaped from the angular portions; whereas the wire when acting as a discharging circuit can exhibit no such appearance. The electricity is then evanescent, and by a law of electrical action determined rapidly toward the negative surface. Many facts might be adduced conclusive of this point, but it seems scarcely worth while to dwell longer on it.

16. The great end which the author proposes to himself in this memoir, is an exposition of the danger attendant on my method of fixed lightning conductors for ships, successfully tried in the British navy for upwards of ten years;—with a view to a substitution of an untried method of his own. It may be worth while, therefore, in conclusion, to see whether the objections he so strongly insists on, do not equally apply to his own conductors as well as to mine, and, in short, to lightning conductors generally.

17. In the first place, he tells us (see 191.) “that it is possible for the most spacious conductor that can be applied to a ship to be rendered sufficiently hot by lightning to ignite gun-powder.”

18. In the next place, he says, (202.) that the “lateral discharge will *always* take place when the vicinal bodies are capacious, and near the principal conductor or any of its metallic appendages.” This was the case, he says, when only his small jar was used, and with this small jar he could produce lateral discharges at a distance of fifty “feet from the direct discharge.”

19. Thirdly, he tells us (203.) that “the magnitude and intensity of a flash of lightning being *infinitely* greater than anything which can be produced artificially, the lateral discharges must be *proportionally greater*,” that is to say *infinitely* great.

20. Taking these data as true then, it follows that any lightning conductor carrying a flash of lightning, would at an *infinite* distance, produce a lateral explosion *infinitely* great, and of course do an *infinite* deal of mischief. Hence, every powder magazine having a lightning conductor, every ship with a lightning chain in her rigging, should whenever lightning struck the conductor be destroyed; for in no case is the conductor at one third the distance from the inflammable matter, of that, at which Mr. Sturgeon can produce a lateral discharge with a jar of “only a quart capacity,” viz. “50 feet.”

21. But Mr. Sturgeon proposes to apply cylindrical copper rods in the rigging; their "upper extremities to be attached to the tops, &c. &c.," "their lower extremities to the chains of the shrouds," and to be united "by broad straps of copper to the sheathing," that is to say, by conductors with edges, which he says throw off the charge into neighbouring bodies; this too after having told us, that the most spacious conductor may become red-hot, and that lateral discharges *always* take place when the vicinal bodies are *capacious*, and near the principal conductor or *any of its metallic appendages*. Under such circumstances what is to become of the rigging, sails, masts? will they not be set on fire? Are not the massive iron hoops and other metals about the masts, the chains of the shrouds bolted through the ship's side, and other metallic bodies in the hull, such as bolts, tanks, chain cables, &c. &c., *vicinal capacious bodies*, and reaching by interrupted metallic circuits up to the very magazines Mr. Sturgeon talks so much about? Must not a ship with such conductors be necessarily destroyed? Surely he must give the British Association and the learned bodies of Europe and America, &c., very little credit for philosophical penetration, if he thinks they will not immediately discard such philosophy as this.

22. Either his "theoretical and experimental researches" are true, and his system of conductors fatal and absurd, or otherwise, if his conductors be good for anything, then his theoretical and experimental researches are good for nothing. He may adhere either to the one or the other, but he cannot have both; such is the *reductio ad absurdum* in which he is involved.

Mr. Sturgeon's anxiety to arrive at conclusions unfavourable to my conductors, has led him to conclusions subversive of *all* conductors, his *own especially*.

23. The mere circumstance of finding his "*third* kind of lateral explosion" decrease in power, by uninsulating his jar, might alone have led him to doubt the accuracy of his deduction. On so important a point, and before he ventured to awaken the prejudices and fears of the uninformed, we had a right to expect at his hands a profound scientific inquiry. He should, at least, have tried whether he could not get this spark after the main charge had passed (*m*) as well as at the *apparent* time of passing. The quantity of electricity should have been accurately measured, and its effects in producing the spark determined, both in relation to the quantity and surface over which it was distributed (*p*). The form and dimensions of the discharging conductor should have been

varied (r). The final electrical state of his apparatus, as also the electricity of the spark, should in common prudence have been examined, (k), together with other manipulations quite inexcusable to neglect on such an occasion. He has however, failed in everything calculated to give value to his inquiries, as I think has been fully shown. They are hence not entitled to the smallest confidence, and it is not a little extraordinary that he should have done so, whilst taking credit to himself for *superior sagacity*, and an acquaintance with facts of which he says I did "not seem to be aware," e. g. the most common-place facts in electricity.

24. In conclusion, I have no hesitation in giving it as my confirmed opinion, after a long and severe examination of the laws of electrical action, and of cases of ships and buildings struck by lightning;—that a lightning rod is purely passive, that it operates simply in carrying off the lightning which falls on it, without any lateral explosive action *whatever*. I do not deny the general inductive effect mentioned by Lord Stanhope on bodies opposed to the influence of the thunder-cloud, and that the displaced electricity will again find its equilibrium of distribution, and return to those bodies, which effect would necessarily take place, whether we had a lightning rod or not (13); an additional reason for linking the detached conductors in a ship's hull into one great mass, so as to have as few interrupted circuits as possible in any direction.

This opinion, by the citation of a few striking cases in which ships have been struck by lightning, I hope in a future paper fully to substantiate, should you think the subject of sufficient consequence.*

APPENDIX.

The author, probably perceiving how little he had gained by quoting Lord Mahon and Dr. Priestley, observes, in a supplementary note, page 235, "Perhaps the experiments of Professor Henry would be more to my purpose." These experiments, however, are no more to his "purpose" than the others, as any one may see who will examine the Professor's communication, in the seventh report of the British Association, page 25. The experiments there described relate to minor electrical discharges, similar to those already mentioned (i). These were obtained by throwing simple sparks

[* We shall be most happy to receive and insert any further communication from Mr. Harris.—Edit.]

from an electrical machine, on small wires or rods, either insulated or connected with the earth: the wires became luminous and the rods emitted sparks. In this case, as Professor Henry observes, the electricity of the machine must be considered as free electricity; and as the bodies on which they fell were all in their natural state, the spark is immediately thrown off as a lateral discharge. Whether insulated or not, the electricity of the body is evidently acted on by induction, before the spark can be distributed over it or the earth. Hence, when sparks of about an inch long are thrown on the upper end of a lightning-rod, or other metallic body passing into the earth, the induction upon the rod and earth requiring a short time for its development, a spark is thrown off upon any adjacent conductor in a state to receive it. Such experiments, therefore, apply only to small quantities of electricity suddenly thrown upon conductors in a neutral state. This, as I have shown, (13, figure 6,) is a distinct case from that, in which a charged surface throws off its redundant electricity upon an opposite surface eager to receive it through a conducting-rod sharing in the electrical state of that surface, and which is consequently prepared already by induction to discharge it. One might be led to infer, from the particular description given by the author of this experiment, page 235, that sparks had been obtained from a lightning-rod at the time of its conveying a discharge of lightning. It may not be amiss to add, that Professor Henry did not consider these experiments as applicable to lightning-rods; and that in accordance with the opinion of Biot, he thinks the spark observable at the time of discharging a jar—that is, Mr. Sturgeon's *new* fact—is entirely owing to a small quantity of redundant electricity always existing on one side of the jar, as I have already stated, (*f*), and not to the whole charge.

I am, Gentlemen,

Yours, &c.

W. SNOW HARRIS.

Plymouth, Nov. 5, 1839.

Westmoreland Cottage,
December 2, 1839.

My dear Sir,

I have read your preceding paper very carefully, under the expectation of finding some close dispassionate reasoning from the pen of one who has so deservedly the reputation of being an indefatigable experimenter in electricity. I expected, also, from the title of your paper, that you would have *inves-*

tigated my fourth memoir, paragraph by paragraph, in the same uniform manner in which they are arranged; pointing out their correctness or incorrectness, in a manly and scientific order. But, although I have been sadly disappointed in this particular, I am yet willing to believe that the next time you attempt to investigate any of the results of my enquiries, your present irritation will have subsided; and that you will see the necessity and importance of keeping *close* to your subject: for no irritated man can be expected to reason well.

I am exceedingly sorry to find that you think I have "laboured hard to invalidate" your experiments which were shown to the Navy Board, at Plymouth, and the British Association, at Liverpool, &c., when no effort of the mind was necessary for the purpose. No electrician need "labour hard" to show the deceptive character of those experiments; nor would it require much effort of the mind to come to the conclusion that those experiments were either *intended* to deceive, or that their author was sadly abroad from his subject. It would be impossible for me to know which side of this dilemma you mean to choose: but I hope you will be enabled to clear up this point and that without delay; for upon *this point* alone hangs much of your credit (which I hope never to see sullied) as an electrician and a philanthropist. My only motive for reviewing your *illustrative* experiments was that of "placing them in a proper light," and I can never expect that you will object to an examination of your illustrations of a topic of such deep interest as that of marine lightning conductors, where thousands of brave men's lives are either to be protected or placed in wanton jeopardy. Think seriously on the importance of this subject before you venture one step farther in your project, and allow candour and experience to be well weighed in your mind on this momentous occasion. No one would have been more delighted than myself had your long paper shown anything like dispassionate controversial argument with close adherence to the subject; instead of which I am sorry to say, you have indulged in blunt and useless asperities which are foreign to scientific discussion, and fatal to the progress of all rational pursuits.

You must excuse my discussing the various parts of your paper individually, at this moment, as my duties press too closely on my time to give them proper attention. I can only now repeat that I am much disappointed at your not touching on the most vital part of my memoir, nor of producing any argument in favour of your favorite plan of marine lightning conductors. In the next number of these

Annals you may expect a full and ample analysis of your paper: at present I will merely offer a few questions for your solution, which, as a gentleman and electrician, you will undoubtedly attend to.

Have I, or have I not, given a fair and candid explanation of your experiments before the Navy Board, at Plymouth? (Fourth Memoir, 176, 177, 178, 179).

Have I, or have I not, pointed out other experiments which, as an electrician, you ought to have made the Navy Board acquainted with in such an important enquiry? (180).

Do you mean to be considered a philosopher, or a necromancer, by endeavouring to persuade the British Association that your blowing asunder two pieces of wood by *gunpowder*, was a true representation of the effects of lightning on a ship's mast? (181).

Have you, or have you not, made any other experiments to show the superior efficacy of your proposed conductors?

Have you, or have you not, made yourself well acquainted with atmospheric electricity by a long series of kite experiments?

To what *kind* of electrical action do you allude the bursting of the iron hoops of the mainmast, &c., of the *Rodney*, and the springing of the nails, and displacement of the lead of "the lantern of the dome" of the *Hôtel des Invalides*?

Which do you think most prudent, to endeavour to lead lightning *into* the ship, or to endeavour to keep it *out* of the ship?

These are plain simple questions, and require nothing more than plain, simple, and unequivocal answers.

I am, dear Sir,

Yours very truly,

W. STURGEON.

To W. Snow Harris, Esq.

P.S. I hope you will perceive that I have no motive in this great question, further than that of eliciting truth and protecting our brave tars from the most formidable of all nature's elements: and you may depend upon my giving you every advantage that these Annals will afford, to support the plan which you have proposed. You will acknowledge that I have hitherto been candid in this particular, by transplanting your paper from another Journal to the Annals; and as it is possible that your letter of the 15th of September may have some weight in your favour, I now offer it to the perusal of our readers. W. S.

Plymouth, September, 15, 1839.

Dear Sir,

I have never received the papers on electricity alluded to in your letter of the 12th instant, and with which I have been duly favoured. I do not think any communications of the kind were received for the Physical Section, of which I was one of the Secretaries at the last meeting of the British Association, at Birmingham ; at least, if they were, I know nothing about it. I cannot understand how any one acquainted with the nature of ordinary electrical discharges, and conversant with the practical results on the great scale of nature, can at all dissent from the simple and plain method I employ for guarding shipping against lightning. However, you seem to think my scheme a dangerous one ; and I will allow that your opinions are entitled to much consideration ; you have entered with considerable ability and skill into electrical actions, and you have my best acknowledgments of your talents. I cannot say as much for those who have been lately engaged in the illiberal crusade against me and my opinions, in London. But as I do not in any way care for, or value what they say, I do not think it worth my while to notice them. Mr. Clarke, Mr. Roberts, with a few ignorant naval men, are quite welcome to visit the Polytechnic daily for the purpose of depreciating my labours, and may publish as many pamphlets for circulation at the different bridges in London as they please. That is a mode of proceeding which must eventually recoil on themselves ; to say nothing of its being unhandsome, illiberal, and uncalled for. I must say I was not a little annoyed at finding you associated against me with others ; since I had always from the time I first met you at Oxford, at the meeting of the British Association, thought we were on better terms ; and that any difference about a philosophical subject might have been settled between us in a better way. However, I cannot help it.

Well now, you say you are about to publish some communications which are to point out the danger of my system of defence from lightning. I cannot possibly have any objection to this. I only hope you will be careful to inform yourself respecting the true state of the question, and not misrepresent me as others have done (unintentionally it will be I have no doubt). You will excuse, I am sure, my saying, if I may judge by your letter, that you have not examined the question faithfully. Let me, therefore, put you in possession of a few points as it may probably save both of us trouble. Like some others you begin by assuming that I have overlooked some important facts connected with discharges of lightning. Perhaps

the contrary may be found to be the case and that those who oppose me have mistaken the road; and I think I see where the mistake made by those who talk of danger from a lateral discharge lies. However, of this more by-and-bye.

I beg you to observe that you are quite wrong in supposing that my conductors pass through the magazines.* Why I never dreamt of such a thing; neither do they exclusively go into the body of the hull; since large metallic bands lead off under the deck to the iron knees, &c., in the side. My object has been to connect all the masses of metal in the hull, and the conductors on the masts into one general system, so as to admit of a general and rapid distribution of the fusing charge without explosion or damage.

You say "there is an apparent intention to introduce my conductors in the navy." Are you not aware of the *fact* of the *conductors having been used in the navy for the last 12 years or more*? Why they have been fitted in six frigates, many line of battle ships, and smaller craft. Men have been exposed to lightning in all parts of the world—South America, Tropics, Coast of Africa, Mediterranean—some have been struck by lightning. I understand in the late inquiry which the government ordered with a view of examining the success of my plan, that extremely valuable evidence has been obtained from naval officers in command of their ships, and from others who have been exposed to lightning under various circumstances. It is, I am told by the Secretary of the Admiralty, very voluminous, will be printed and laid before Parliament. I do not know the amount of it myself, but I think it would be as well to examine the documents before we enter upon the public discussion you have marked out; as to mere opinion it will go for nothing any way; and you must go to facts. Allow me to call your attention to the *Nautical Magazine*, No. 2, for February last, 1839, for the actual effects of lightning on three ships of the navy; and if you will go to *Mr. Payne*, at the *Polytechnic*, he will show you the diagram I left there, illustrative on a large scale of those effects. Tell me, where was the calorific and lateral discharge to which you allude in this case? If we could meet and examine this subject together experimentally, I believe we should soon settle the difference. Whatever I may be induced to do by way of reply to anything you advance, will be simply an appeal to facts. I possess a great body of evidence from a history of

* Will Mr. Harris say that not one of his conductors passed through the powder magazine of H. M. S. *JAVA*? See Lieut. Green's Letter, p. 329. Edit.

cases of lightning on ships which bear out my views; but I certainly shall not write anything until I see the result of the inquiry and investigation lately instituted by the Admiralty. I think you would do well not to advance anything without a pretty close appeal to experience. I shall always consider that I have been illiberally treated by many persons in this affair, as you would say if you knew all.

I am, dear Sir,

Yours faithfully,

W. SNOW HARRIS.

To W. Sturgeon, Esq.

XL. *On the effects of Lightning on H.M.S. Beagle.*
By LIEUT. SUTWAY. *In a letter to the Editor.*

Hushing, near Falmouth,
October 9, 1839.

Sir,

Having considered your communication in the *Annals of Electricity*, on marine lightning conductors, containing observations on the stroke of lightning which fell on the masts of H.M.S. Beagle, I think it fair, both to Mr. Harris and the naval service, to describe the phenomenon I witnessed on that occasion; first stating that at the time of my joining the Beagle in 1831, previously to her leaving England, I had no acquaintance with Mr. Harris, and certainly no *bias* in favour of the conductors with which the ship was fitted. I may, therefore, claim to be considered an impartial observer.

At the time alluded to, I was first Lieutenant of the Beagle, and was attending to the duty on deck. She was at anchor off Monte Video, in the Rio de la Plata, a part of the world very often visited by severe lightning storms. Having been on board H. M. Frigate, Thetis, at Rio Janeiro a few years before, when her foremast was totally destroyed by lightning, my attention was always particularly directed to approaching electric storms, and especially so on the occasion alluded to, as the storm was unusually severe. The flashes succeeded each other in rapid succession, and were gradually approaching; and I was watching aloft for them when the ship was apparently wrapt in a blaze of fire, accompanied by a *simultaneous* crash, which was equal, if not superior, to the shock I felt in the Thetis; one of the clouds by which we were enveloped, had evidently burst on the vessel, and as the mainmast appeared for the instant to be in a mass of fire, I felt certain that the lightning had passed down the conductor on that

mast. The vessel was shaken by the shock and an unusual tremulous motion could be distinctly felt; as soon as I had recovered from the surprise of the moment, I ran down below to state what I saw and to see if the conductors below had been affected, and just as I entered the gunroom, the purser, Mr. Rowlett, ran out of his cabin (along the beam of which a main branch of the conductor passed), and said that he was sure the lightning had passed down the conductor, for at the moment of the shock he heard a sound like rushing water passing along the beam. Not the slightest ill-consequence was experienced; and I cannot refrain from expressing my conviction that had it not been for the conductor, the results would have been of very serious moment. This was not the only instance, when we considered that the vessel had been saved from being damaged by lightning by Mr Harris's conductors; and I believe that in saying I had the most perfect confidence in the protection which those conductors afforded us, I express the opinion of every officer and man in the ship; and as Captain Fitzroy's opinion must have much greater weight than mine, from his superior knowledge on the subject of electricity, I cannot refrain from copying his opinion of the conductors in the *Beagle*, which is published in his appendix to the *Beagle's* voyage.

"Previous to sailing from England in 1831, the *Beagle* was fitted with permanent lightning conductors invented by Mr. W. S. Harris, F.R.S.

"During the five years occupied in the voyage she was frequently exposed to lightning but never received the slightest damage, although supposed to have been struck by it on, at least, two occasions; when at the moment of a vivid flash of lightning, accompanied by a crashing peal of thunder, a hissing sound was heard on the masts, and a strange though very light tremulous motion in the ship, indicated that something unusual had happened.

"The *Beagle's* masts, so fitted, answered well during the five years' voyage above mentioned; and are still in use on board the same vessel on foreign service.

"Even in such small spars as her royal masts and flying jib-boom, the plates of copper held their places firmly, and increased rather than diminished their strength.

"No objection which appears to me valid has yet been raised against them; and were I allowed to choose between having masts so fitted and the contrary, I should not have the slightest hesitation in deciding on those with Mr. Harris's conductors.

"Whether they might be further improved, as to position and other details, is for their ingenious inventor to consider

and determine. He has already devoted so many years of valuable time and attention to the very important subject of defending ships against the stroke of electricity, and has succeeded so well for the benefit of others, at great inconvenience and expense to himself, that it is earnestly to be hoped that the government, on behalf of this great maritime country, will, at the least, indemnify him for time employed and private funds expended in a public service of so useful and necessary a character."

Not being sufficiently acquainted with electrical experiments, I cannot remark upon those you have adduced in support of your opinions detrimental to Mr. Harris's conductors. I can, therefore, only repeat my conviction that the *Beagle* was struck by lightning in the usual way, and certainly without any *lateral explosion* or other ill effects, similar to those you insert in your *Annals of Electricity*.

I am, Sir,

Your obedient servant,

B. T. SUTWAY,

Lieut. R. N.

Observations.

Lieut. Sutway's description of the lightning rods on the *Beagle* is obviously of a very different character to that given by Capt. Fitzroy, and certainly much more favourable to the idea of the ship being struck than given by the latter officer. I consider Lieut. S's description of the occurrence exceedingly valuable; for it is the minute detail of the effects of lightning that we are most in want of, and it is much to be lamented that our data on this momentous topic is yet so scanty. There is nothing, however, in this letter that can in the least affect my statements regarding the electrical principles that would be brought into play by flashes of lightning striking vessels.

W. STURGEON.

XLI. *On Mr. Snow Harris's Lightning Conductors, as applied to Shipping. In a letter to the Editor. By W. PRINGLE GREEN, Lieut. R.N.*

1, James Street Adelphi, July 18, 1839.

Sir,

An important epoch has arrived in practical electricity by the Government appointing a committee to determine on the subject of fixed conductors, fitted to the masts of several of

her Majesty's ships, passing through the hull and after-magazine. Having in the year 1822, on the scheme being introduced into the Navy, by order of the Navy Board, opposed its adaptation, upon incontrovertible evidence, I am again prepared to show, its being an ill-copied plan of Mr. Marrot, published in 1812, in the *Naval Chronicle*, Vol. I, p. 201, and the extreme danger of such conductors, proved, by experiment, and a mass of electrical phenomena; and by my representation of these facts the then existing Board of Admiralty countermanded the N. B.'s order. As this plan has been introduced into the Navy, and the necessity of investigating a matter of such vital importance to the state at this time, needs no comment: as I do not believe it possible otherwise than by a perusal of the account of experiments made at Plymouth; of my queries and experiments; and a review of my researches during 35 years in every quarter of the globe, illustrated by drawings, for the most experienced theoretical electrician to give a correct decision. I am, therefore, desirous to put you in possession of the whole of this matter, upon which I take my stand.* At this moment the subject acquires a great interest throughout the Naval Service, and very gross deception has been, and continues to be, practised upon that service and the public, at a heavy cost to the nation, by making experiments which seems to demand the protection of the public press.

I have the honour to be, Sir,

Your obedient servant,

W. PRINGLE GREEN,
Lieut. R. N.

Lieut. Green's Queries.

1. Will not the superabundant electric fluid from the spindle in the truck, which passes six inches into the body of the mast, explode and destroy it?

2. How is the spindle to be substituted when the top-gallant mast is on deck, which is generally the case in stormy weather, the cap has much iron about it this being the highest point? It is not possible to place the spindle and connecting copper across the cap, without being in contact with much iron about it. Will not this iron draw off the fluid and cause an explosion? And will not the nails in the copper strips

* We have in our possession much valuable information on this subject from Lieut. Green, but only give a few of that Officer's queries in this place. Edit.

do so? If these conductors, such as the proposed, are sufficient safeguards, when passing through a ship, how is it that Heckenham Poor-house was set on fire though it had eight of the largest and most approved conductors placed on the outside; and what must have been the result had they passed through the building? As conductors can be surcharged, broken, and fused, and electric fluid becomes sensible in the form of a spark upon the surface, and, as it has been shown by experiment, streams of flame are sometimes conducted along the surface of a conductor; are not these facts alone sufficient proof, that it is dangerous to conduct these electric streams through a ship's *powder magazine*? When the electric fluid is sensible in the form of a spark or sparks, or in such streams, and conveyed by the conductor to the inflammable air in the bottom of a ship, will it not cause ignition of this inflammable air and burn the ship? Hydrogen is put into a gaseous state by the agency of electricity, and the bilge water would be decomposed into oxygen and hydrogen gas and instantly blaze. Will not the electric sparks which form upon the conductors pass off to the iron tanks and iron ballast, and may it not explode under the powder magazine where it is conveyed by iron ballast?

3. How is it to be presumed that conductors, such as the proposed, can guard a ship from a stroke of lightning when it is known that a single flash fused a conductor on the main-mast, shivered the foremast, splinters distant parts of the deck, and a sufficiency of the electric fluid passes down below, destroying bulk-heads and fusing a bar of metal there. The spare topmasts and topgallant masts being fitted with conductors, and placed in the centre of the ship, as is the custom in all her Majesty's ships between the fore and main-masts, pointing both to the quarter-deck and forecastle, on which the officers and crew are always in considerable numbers. Will not these people be killed by a discharge from these longitudinal conductors?

4. Should the conductor convey any portion of the fluid to the bolts in the keel touching the copper sheets on the bottom, will it not pass along the bottom and knock off the remainder, and will not these bolts be driven out and the keel split? What will be the expense to complete the Navy with such a scheme? Upon a very moderate estimate it will require £500. to dock and complete each ship, and £300,000. be required to complete the whole Navy. That there is not the smallest difficulty attending the hoisting up a chain conductor, it is a fact, for one man and a boy can accomplish this; and when up and fastened to the back-stays, it cannot

be injured though it remain up for a voyage. In what does the plan differ from Mr. Singer's, proposed nine years previously, who first put bolts through the keel, or those thirty years ago in use in the French Navy; both being abandoned by the inventors as chimerical and dangerous in the extreme.

As long back as Capt. Cook's being at Java, in the *Endeavour*, in 1769, the dangerous effects of spindles in the mast of ships are recorded by Mr. Green who accompanied him. During a storm of thunder and lightning and rain, the mast of a Dutch Indiaman was split and destroyed from the spindle to the deck. So great was the shock, that considerable fear was entertained for the safety of the *Endeavour*, as the explosion shook her like an earthquake; proving that it is not only the ship or building to which a conductor is affixed, that is endangered, but all for a considerable distance around. If a single spindle can invite so powerful and dangerous an agent, how much greater must be the stroke in presence of three of them such as are placed in the masts of her Majesty's ships, in ordinary at Plymouth. A link of the chain conductor such as used at sea is put over the massive spindle and continued to the water. The first experiment made was to prove the danger of such a scheme. A model being produced, and the spindle exposed to an ordinary discharge of the fluid from the battery, the chain was instantly fused by the lateral discharge, and the mast splintered, proving the danger of the plan, and that the strips of copper of the fixed conductors to the masts of ships would be fused from the spindle.

A mast thus splintered and set on fire would involve the ship in destruction. It has been asserted by the suggester of this scheme, that although the fixed conductor had been cut through by a saw, or a break made in it, this would not impede the passage of the fluid. "That sparks were passed through gunpowder without igniting it, and that electric fluid is always transmitted along the surface of conductors."

2nd. Ex. A copper conductor was passed through the centre of the magazine of a ship fitted after the plan in question, *precisely as H. M. Ship Java was fitted*, with the exception of it being nailed to the mast. The conductor was cut through to represent the break said to be made in the one fixed to the mast of the cutter, in the public experiment, powder placed near to the fracture, in a shock being passed through the conductor instantly ignited. A greater charge was then sent through the conductor which was instantly melted, globular metal being produced. Several other experiments were made to exemplify more satisfactorily the *fusion of conductors* by lightning.

XLIII. MISCELLANEOUS ARTICLES.

Letter to the Editor of the Annals of Electricity, &c., &c.
From the REV. N. CALLAN, Professor of Natural Philosophy.

Maynooth College, Nov. 11, 1839.

Dear Sir,

I have read, within the last week, a letter from Professor Forbes, of Aberdeen, to Dr. Faraday, in which he states that Mr. Davidson, of Aberdeen, has been eminently successful in the production of a moving power by electro-magnetism; and that Mr. Davidson "is the first who employed the electro-magnetic power in producing motion by simply suspending the magnetism without a change of the poles. This, he says, Mr. Davidson accomplished about two years ago." I believe I may fairly dispute Mr. Davidson's claim to be the first who employed that method of applying electro-magnetism as a moving power. It is about two years since I first constructed an electro-magnetic engine for the production of motion, in which there was no reversion of the poles of the magnets, but only a suspension of their magnetism. In a letter of mine, dated February 20, 1838, and published in the *Annals of Electricity*, on the first of April, in the same year, I refer to three different electro-magnetic engines which I had then made. In one of these there was no reversion of poles; but only a suspension of the magnetism. I have since made several engines on the same principle. I made one in August, 1838, for the Right Rev. Dr. Carew, Coadjutor, Bishop of Madras, which he brought with him to India, for the use of his Seminary. In this there were two magnets; the motion was produced not by a reversion of poles, but by a suspension of the magnetism. In the commencement of the present year, I made one on the same principle for the College. It worked very well, although it contained only a single magnet: it was exhibited to my class last February. Within the last two years I have made a great variety of machines for the production of motion by electro-magnetism. In some of these the poles were reversed; in others, the magnetism was only suspended; and in others, the magnetism was constantly maintained. In some the motion produced by the magnetic force was rectilinear, and, of course, a crank was employed to convert the rectilinear into a curvilinear motion: in others, a rotary motion was directly produced by the magnets. The crank engines differed from all the crank engines of which I have seen any description, in this respect, that the length of the stroke might be ten or twenty feet if necessary,

without any diminution of the *moving* power of the magnets. The immediate publication of the results of my experiments would be premature as some still remain to be made. I intend soon to make an engine of considerable power. My experiments give me every reason to think that, with a given battery, the moving force of each magnet, in that machine, will be at least eight or ten times as great as it would be if the magnet were placed in an engine constructed on the plan of Professor Jacobi. I do not know the plan of Mr. Davidson, and therefore cannot compare it with mine. The moving force which he obtained appears to me very small, when I consider the size of the battery employed. However, that may have arisen from a defect in the construction of the machine rather than from any defect in his plan. He certainly deserves encouragement. I agree with Professor Forbes that "it would be much for the interest of railroad proprietors," and still more for the interest of companies who use stationary engines, to take up the subject; and to incur the expense of making experiments, on a large scale, on the best method of applying electro-magnetism to the working of machinery. I am fully convinced by the experiments of Professor Jacobi and of Mr. Davidson, and still more by my own, that electro-magnetism will ere long be substituted for steam. I intend to send you, as soon as convenient, an account of the principal experiments which I have made for the purpose of ascertaining the best means of employing electro-magnetism as a moving power.

I have the honour to remain,

Your very obedient humble servant,

N. CALLAN.

Method of distinguishing the Arsenuretted, from the Antimoniuretted Hydrogen Gas. By PROFESSOR MAX.

The metalluretted hydrogen gas for examination being ignited at a jet, a piece of porcelain is held over it till a dark speck is produced. The speck is then moistened with a drop of nitro-muriatic acid. Then by adding a drop of the aqueous solution of sulphuretted hydrogen a precipitation takes place. If the tested gas be arsenuretted hydrogen the precipitated substance is of a *pure yellow colour*: but if it be antimoniuretted hydrogen the precipitated substance is of a *deep orange colour*. The difference of colour is so distinct that no one can mistake the one from the other. The whole process is exceedingly simple and may be performed in a few

minutes. *Poggendorff's Annalen des Physic and Chemie*, No. 3, 1838.

The following method has lately been given by Mr. Marsh. The piece of glass or porcelain intended to receive the metallic crusts is to have a drop of distilled water placed on it: and then, with the drop of water on the lower surface, held a little above the apex of the cone of flame of the burning gas which is issuing from the jet. If arsenic be the metal in the hydrogen, arsenical acid formed by this process is dissolved by the drop of water, and is easily detected by a drop of the ammoniacal nitrite of silver, which immediately produces the arsenite of oxide of silver, which is of a lemon yellow colour. If antimony be the metal in combination with the hydrogen gas under examination, no such result is produced by this process. When much arsenic is present it will be advantageous to employ a clean glass tube, about six inches long, and slightly moistened inside with distilled water. The tube, thus prepared, is to be held vertically over the flame of the burning gas, and a strong solution of the substance is soon obtained, which may be tested as before stated.

My dear Sir,

Your correspondent, C. Barker, Esq., has noticed my question in your Annals for June 1838, respecting the spotted jar. If he did not observe the appearance of the sparks as I then stated, it is possible I may have been mistaken; it is many years ago since I noticed what I remarked. To save the trouble of fixing spots on the inside of a Leyden jar, I lined it with plain tinfoil, and on the outside fixed very small spots the size of those used for spiral tubes. Now, the pieces of tinfoil being so diminutive, they could hold but a small portion of electricity, and therefore the spark might be almost imperceptible. I regret not having leisure to try another jar lined with plain tinfoil, for, if it answers, a great saving of trouble is effected.

I feel greatly obliged to Mr. Barker for reminding me of this circumstance, as it gives me an opportunity of correcting a mistake; and also for the valuable information contained in his letter inserted in the last No. of your Annals. His recommendation of an insulated stand is well deserving the attention of all experimenters; I have used it for several years to exhibit the electric fly, orery, dancing figures, &c. but lately it occurred to me that a resinous plate about 12 inches

in diameter, and a third of an inch thick, will answer all the purposes of an insulated stand.

Mr. Barker enquires how to line the inside of large carboys, to make them Leyden jars, which you have informed him, but I think he will find that although green glass does very well for electrical machines, it will not answer for Leyden jars; I once constructed a battery of them which proved useless.

I am,

Very truly yours,
J. HARPER.

Oxford, Dec. 26, 1839.

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

MARCH, 1840.

Considerations on Chemical Forces ; By M. GAY LUSSAC.—
*FIRST MEMOIR on Cohesion.**

I purpose presenting, successively, in several memoirs, some reflections on affinities; this subject appears to me one of great interest, but it is very difficult, and in entering upon it I should wish to reckon upon the indulgence and favorable concurrence of chemists.

In the year 1718, a time when chemistry was yet obscure, Geoffroy, the elder, endeavoured to classify bodies according to the chemical relationship observed between them. He established the proposition, that, *whenever two substances which have a disposition to join together, are found in connexion, if a third which has a greater inclination for one of them, approach, it will unite to that, and cause it to abandon the other.*

For the support of this proposition, Geoffroy made a very simple table of the relationship between the different substances, then known. It is printed in the *Memoirs de l'Académie royale des Sciences*, for the year 1788, page 202; but I thought it would be interesting to reproduce it here, as a historical monument, such as Geoffroy has given, replacing the chemical symbol of each substance by its proper name.

* From the *Compte Rendus*; translated by J. H. Lang, Esq.
VOL. IV.—No. 23, *March*, 1840. AA

Ardent Spirits.	Acid or Marine Salt.	Nitrous Acid.	Vitriolic Acid.	Absorb- ing Earths.	Salt fixed Alkali.	Salt Volatile Alkali.	Metallo Sub- stances.	Mineral Sulphur.	Mercury	Lead.	Copper.	Silver.	Iron.	Antim.	Water.
Salt fixed Alkali.	Tin.	Iron.	Only principle primitive Sulphur.	Vitriolic Acid.	Vitriolic Acid.	Vitriolic Acid.	Acid of Marine Salt.	Salt fixed Alkali.	Gold.	Silver.	Mercury	Lead.	Antim.	Iron.	Spirits of Wine and Ardent Spirits.
Salt volatile Alkali.	Antim.	Copper.	Salt fixed Alkali.	Nitrous Acid.	Nitrous Acid.	Nitrous Acid.	Vitriolic Acid.	Iron.	Silver.	Copper.	Calamin Stone.	Copper.	Silver, Copper, Lead.	Silver, Copper, Lead.	Salt.
Absorb- ing Earths.	Copper.	Lead.	Salt volatile Alkali.	Acid of Marine Salt.	Acid of Marine Salt.	Acid of Marine Salt.	Nitrous Acid.	Copper.	Lead.						
Metallic Sub- stances.	Silver.	Mercury	Absorb- ing Earths.		Spirit of Vinegar.	Spirit of Vinegar.	Spirit of Vinegar.	Lead.	Copper.						
	Mercury	Silver.	Iron.		Mineral Sulphur.			Silver.	Zinc.						
			Copper.					Antim.	Antim.						
			Silver.					Mercury							
	Gold.						.	Gold.							

The substance, at the head of each column, is compared with those beneath in a decreasing order of affinity. Thus, in the first column, ardent spirits (or acids) have a greater affinity for fixed than for volatile alkali salt, absorbing earths, and metallic substances. In the fourth column, it is the oily principle, or primitive which has the greatest affinity for the sulphuric acid : afterwards come the fixed, and volatile alkali salts, alkali salt, absorbing earths, iron, copper, and silver.

Examining the different relations expressed in each column of the table, we perceive that Geoffroy, has confounded the effects of affinity, which ought to have been separated from one another, and has compared some things which are not comparable. Thus the decomposition of sulphuric acid by the pretended primitive sulphur, iron, copper, and silver, cannot be assimilated to the affinity of this acid for their bases. But this is not surprising, as in the time of Bergman, half a century later, the same confusion still existed. Geoffroy, had not accompanied his table with any explanation, he was limited in making the application of it to the preparation of corrosive sublimate by several processes, and he has done it in a tolerably successful manner. Geoffroy's table, notwithstanding its imperfections, is a fine conception ; it is also the first progress made in philosophical chemistry.

It appears that for some time but little importance was attached to Geoffroy's table of relations. Subjected to several disturbing causes which often made them vary, they were considered as vague, indeterminate, and entirely dependent upon circumstances.

But Bergman, thinking that all the operations of chemistry, synthesis or analysis, were founded upon attractions which were not understood, because they were subjected to certain conditions, which incited, stopped, or disturbed them, at last drew the attention and interest of chemists to the causes of chemical phenomena, and his dissertation, *De affinitatibus electivis*, published in 1775, also fixed a remarkable epoch in the history of the sciences.

Bergman distinguished in a body the attraction of similar molecules, which he designated by the name of *attraction of aggregation* ; and the attraction of heterogeneous molecules, which he calls *attraction of composition* ; when this acts so that one substance displaces another in a compound, it then takes the name of *simple elective attraction* ; and if it act between two compounds, whose elements may be reciprocally changed, it takes that of *double elective attraction*.

Notwithstanding the opinion which some chemists had of the inconstancy of affinities, Bergman appeared to consider

them as absolute, determined forces, but whose effects could be modified by certain causes, the influence of which he often appreciated in an ingenious, though sometimes very incomplete manner.

The first of these causes he found in the difference of volatility of substances presented in the same sphere of action. Bergman conceived that the difference in the affinity of two substances for a third, at a given temperature, might be more than compensated at a higher temperature, by a difference of volatility in favour of the substance which had less affinity than the other, but more fixity.

Before Bergman, the results of the affinity between three substances were confounded with those in which there were four, that is, the products of the simple elective affinities with those of the double affinities; and as they are really very different, an objection was raised, from this circumstance being badly comprehended, to the theory of affinities. Thus, from the table of Geoffroy, fixed alkalies have a greater affinity than lime for acids, since in fact they separate it from the gypsum. However, it is said, that if we dissolve chalk in aquafortis, and add a solution of vitriolated tartar, the gypsum regenerates immediately; a proof that the calcarious matter here manifests a greater power. Bergman remarks with propriety, that the two circumstances are very different, since in one there are only three, while in the other, there are four substances present. He explains the reproduction of the gypsum in the mixture of nitrate of chalk with sulphate of potassa, from the double elective affinities, conceiving the sum of the two active affinities to be greater than that of the quiescent. The explanation is certainly ingenious, but at present it is not enough.

The effects of affinity may, according to Bergman, be further disguised, and the theory blamed, by unexpected alterations in the substances present:—for example, nitric acid separates marine acid from its alkaline base, a fact which has been known for some time; but Mayraf has discovered that marine acid can, in its turn, displace nitric acid in salt-petre. While ignorant of the true nature of marine acid, says Bergman, this reciprocal displacement of one acid by another has escaped all explanation; but now we know that marine acid contains some phlogistic, all difficulty vanishes. Nitric acid displaces muriatic by simple affinity; the latter yields its phlogistic to the nitric acid, whether it be free or combined with a base, and hence their reciprocal displacement becomes the consequence of this alteration. It is also thus that white arsenic (arsenious acid) decomposed by distillation salts

formed by nitric, but not those formed by marine acid, because they also contained a certain quantity of phlogistic.

Bergman equally well explains the anomalies of decomposition due to solubility. It happens, says this illustrious chemist, that at first no trace of decomposition appears, though it has really taken place; thus vegetable, displaces mineral alkali from its combination with acids, although we perceive no conglomeration, no precipitate; this circumstance has caused some celebrated chemists to conclude that vegetable, has no power over mineral, alkali. But let us suppose that a little of this latter has been eliminated, is it separated?—certainly not; it remains in solution: for if we evaporate it we shall obtain *crystallized* mineral alkali, with which we could produce Glauber's salts or quadrangular nitre.

I shall here conclude these quotations. They are sufficient to shew that Bergman has deeply studied the theory of affinities, and enriched it with numerous and useful observations. What he says of simple elective affinities is in general exact. The imperfections that we remark in it, extend even to the state of the science, still uncertain and often obscure in its march; and perhaps the *Chemical Statics* has made us too quickly forget the real services Bergman has rendered to philosophical chemistry.

In what concerns double elective affinities, with the equilibrium of active and quiescent forces, Bergman has certainly shewn great clearness: his explanations are seducing; but he has not understood the correct explanation of the precipitates, obtained by the concurrence of double affinities.

Bergman, in imitation of Geoffroy, has explained nothing concerning the measure of affinities, and he was right. This question is even now a delicate one, and not at all easy of access. He has confined himself to arranging the bodies by their greater or less affinity.

Such were the ideas of Bergman on affinities, which were prevalent 'till Berthollet published his researches on affinity, and his *Chemical Statics*, but, they were then eclipsed by the great eclat thrown on these two productions.

Berthollet, in the study of affinities, was engaged with two principal ideas: the influence of the force of cohesion in chemical phenomena, and the proportion of affinities, which he thought to find in the mass of bodies that enter into combination.

According to this illustrious chemist, cohesion or reciprocal attraction of similar molecules is a powerful force which can balance the affinity of heterogeneous molecules, and determine combinations and decompositions.

It does not exist at the time it shows its effects only, but even a long time before it becomes effective. He shews from this analogy, that as soon as a liquid becomes gaseous, and a gas liquid, the dilation of the former, already influenced by the gaseous state it is about to take, and the contraction of the second, influenced by the liquid or solid state it is taking, follow a progression more rapid than the greatest distance of this term. But this argument of Bergman for the establishment of the influence of cohesion, some time before its effects are manifested, remains without foundation, when we consider that there is no unique, constant point for the change of a liquid into an elastic fluid, and reciprocally; but on the contrary, this change is incessant at all temperatures and under all pressures.

Whatever opinion others may form from Berthollet's demonstration, it is sufficient for me to state that he adopts the pre-existing influence of cohesion, and that he has made it enter into all the precipitations and chemical solutions. The affinity, says he, which may produce the solid state, ought to be considered as a force which not only acts when the solidity appears, but even before that time; so that every time he produces a solid substance, whether by separation or combination, we must look in the reciprocal action of the parts which acquire solidity for the cause itself which produced it, although it may not be manifested before.

The theory of decompositions has received from Berthollet unexpected improvements. We are indebted to him for the principle that the change of acids and bases between two salts takes place, every time the salts proceeding from the change or only one of them, have less solubility than the given salts. This principle is of a fortunate fertility, and, we may say, constitutes one of the finest acquisitions of chemistry. But Berthollet taking cohesion for the first cause of double decomposition, does not appear to me to have given the true demonstration of it. He supposes it is the cohesion of the salts not yet existing, which nevertheless determines their formation, and this supposition is inadmissible. For if we can agree with him that the cohesion begins to act in the solution of a salt before the time of crystallization, there is no more of it even when the salts do not exist any longer, as in the case of the mixture of two saline solutions.

Bergman supposed that affinity was an absolute force, admitting no division in its effects, and had only established among bodies a relative affinity. Berthollet, on the contrary believed that affinity was not used in an absolute manner, without division; that thus a base in the presence of two acids

did not exclusively combine with the most powerful, as Bergmen thought; but that it divided itself between the two in proportion to their affinity and quantity. Hence the principle of Berthollet, *that the affinity of different acids for the same alkaline base, is in the inverse proportion to the ponderable quantity of each of them, which is necessary for the neutralisation of an equal quantity of the same alkaline base.* At present, and, I may say, for some time past, this measure of affinity has been abandoned; At the time Berthollet wrote his *Statique chimique*, the atomistic theory was but little understood, and some years later, Berthollet would certainly not have proposed a method of measuring affinity that gives nothing else but the atomic weights or equivalents, which we know to be independent of chemical attractions, or at least to have but very distant connexions with them. I hope hereafter to be able to return to this subject, as well as to the division of one substance between two antagonists. For the present I shall confine my observations to the force of cohesion, since it is made to take so large a share in most chemical phenomena, and as it is of the utmost importance to the better appreciating its real influence.

The attraction of heterogeneous molecules has been rightly distinguished after Bergman, from that of homologous or similar molecules, which has also been designated by the name of *aggregation*, and since Berthollet, by cohesion. These two forces have, without doubt, the same origin, but not appearing to have any common tie in different bodies, their effects could not be confounded.

Cohesion itself takes different names from the lights in which we consider it. It is called *tenacity*, when weight or force is opposed to it for determining the rupture of a body. It is called *hardness*, when taken for the resistance one body offers to another with which we wish to cut it. *Tenacity, and hardness* are evidently *cohesion* itself; or at least they both essentially depend upon it. Bodies which have the most tenacity are also generally those which have the greatest hardness or, according to our notions, the most cohesion. Nevertheless this ought only to be understood of uncrystallized bodies, because, for crystallized bodies, there exists an easy cleavage, and we can very well conceive that there may exist notable differences between *hardness and tenacity*, according to the direction of the rupture and separation of the particles.

Comparing among themselves the three states which the same body can take, we have been led to make each of these states depend on the relation of the peculiar cohesion of the molecules of this body to their repulsion. It is very certain that

in solids the cohesion is the greatest ; in liquids it is much less, but it is always something, since there is no liquid which does not take the globular form, and that a drop suspended from a solid, may be divided into two parts, of which the lower adheres to the higher, notwithstanding the weight which inclines it to fall.

The word *cohesion*, in a chemical point of view, is taken under another acceptance. Here the action is complex ; the body to be dissolved and the solvent are both present, and each acts on the other. The resistance the one offers to the other is called *insolubility*, which must never be taken but in a relative sense. This resistance, let us now say, insolubility, depends essentially, according to the established belief, both on the cohesion or reciprocal attraction of the similar molecules of the body to be dissolved, and its affinity for the solvent we present to it. So that it is supposed, if the body, instead of being solid, were liquid, the solvent would take a much more considerable quantity of it.

This, if I mistake not, is the opinion commonly formed of chemical cohesion and solution. Not being able to divide it nicely, and proposing to discuss it, I thought I ought here to introduce these details, which their shortness will without doubt excuse. The progress of science brings daily new modifications into our ideas, and it is very necessary to fix the starting point of a discussion, if we wish it to be clear and useful.

But before treating of cohesion with regard to its influence in chemical phenomena, I shall allow myself to turn my attention to a physical operation which also appears connected with cohesion, and appears to me very proper to throw light on the method of influence of this force, I speak of volatilization.

I suppose a volatile body, able to present itself under the solid and liquid forms within the limit of temperature accessible to observation ; water for instance. If the elastic force of its steam be determined, starting from 20 below zero, at which it is solid and possesses a great cohesion, we find that the progression of this elastic force, is no way affected by the passage from the solid to the liquid, or, reciprocally, from that part of the liquid to the solid state, that is to say, that the elastic force of ice at zero is precisely the same as that of water, at the same temperature ; a similar observation for every other degree of the thermometer at which we can find water at the same time in the solid and liquid state, the elastic force of steam will remain the same for both ; and, however, without exactly deciding the degree of cohesion of ice, in com-

parison with that of water, we may admit that it is incomparably greater, perhaps more than a thousand times.

This observation which struck me some time ago I have verified on hydrocyanic acid, which we know solidifies at about 15 below zero and still preserves a very great volatility. The progression of the elastic force of its steam, has been no way affected at the time of the change of state; and this result may be considered as general.

Hence there is no relationship between the cohesion or attraction of the molecules of a body and their repulsive force; the one is consequently quite independent of the other and the elastic force of steam is only determined by the number of molecules able to maintain themselves in a gaseous state, in a limited space at a given temperature.

However when we consider that salt water produces a steam whose tension is less than that of pure water, at the same temperatures,* a result which can only be explained by an affinity of the aqueous for the saline molecules, we may ask, in assimilating this affinity to that of water for its own molecules, if the space above a surface of water is really saturated with steam, that is to say, if the equilibrium established, the least cooling of the steam taken from the action of the water, the least reduction of space would not occasion the precipitation of a certain quantity of steam; or whether, for the same space above salt water, the saturation is not complete, so that the steam taken from the action of the liquid might be cooled or reduced in volume within certain limits, without the least precipitation of its molecules. I am disposed to believe that the space above pure water becomes completely saturated with steam, from the consideration that the difference of the attraction of the molecules of ice among themselves, to that of the molecules of water, avails nothing in the elastic force of the steam of each of these bodies, taken at the same temperature. Nevertheless, the experiment does not appear to me less interesting to try, and although very delicate, I propose to prepare for the execution of it.

The observation that the elastic force of a body remains constant at the instant of the change between the liquidity

* It has been pretended that the steam which comes from an aqueous saline solution, boiling later than water (at 110° for instance) was always at 100°. This is a very great error; steam has always the same temperature as the last liquid bed it traverses; but what has caused the deception is, that steam, as also every other elastic fluid, cools very rapidly until the time of their condensation, when the cooling is more powerfully compensated by the liberation of their latent caloric.

and solidity, doubtless clashes with the received ideas relative to the molecular constitution of each of these states ; but it would not oppose them less even if we derived from it the consequent, that molecular attraction is the same for a liquid as for a solid at the instant of the change of state ; for this is accompanied with variations as much in the volume of the body as in its quantity of caloric, which appears to announce a great alteration in its molecular constitution. And whether the molecules in taking the solid state, are only caused to approach ; whether they are placed together otherwise ; or finally whether they unite in small geometrical groups which, by their arrangement, would modify the volume of the body ; results all of which, depend necessarily on another mode of action in the molecular forces ; at least it is certain from our scientific analogies, that they are then in very different conditions from what they were before the change, and it is still very remarkable that their elastic force is indifferent to all these perturbations.

These preliminaries established (and I consider them of great importance from their connexion with the principal question which I have started) I shall turn my attention to the effects of cohesion, and follow them up more particularly in solutions.

We will look for some bodies uniting the double condition of being soluble in a solvent, and of being able to appear solids and liquids within accessible limits of temperature for the determination of their solubility.

Among salts I do not know of any which combine these two conditions.

Among acids, I thought camphoric acid, of which we find a table of solubility in Berzelius, from Brandes, would furnish me with an example of solubility under the desired conditions ; and in fact this acid whose fusibility is given at 63° , appeared to show a solubility above and below this point, which was subject to a law of regular continuity. But wishing to repeat these experiments of Brandes with some camphoric acid, such as is obtained from M. Leibey, I perceived that this acid would not fuse even at 300° , and consequently I abandoned it.

Among inflammable bodies, cetine, paraffine, fat solid acids, present no anomaly in their solubility in alcohol while passing from the solid to the liquid state ; the progression in proportion as the temperature increases is perfectly continuous and regular. I shall give by and by these different solubilities, regretting, much, that I have not among the salts more conclusive examples.

But the cohesion of these different bodies while they are solid being greater than when they are liquid, and their solubility not being disturbed at the instant of changing from one state to another, neither before nor after it is absolutely necessary that it be independent of the cohesion.

Further, if I take the solubility of an oil in alcohol I find that it acts in general precisely like that of a solid, although liquid, that is to say without great cohesion; the solubility, very feeble at a low temperature goes on increasing progressively with it. Thus a body whether it remains constantly liquid, or whether at first solid it afterward becomes liquid, presents under each of these circumstances the same kind of solubility.

Gaseous substances themselves such as chlorine, do not seem to me to undergo any alteration in the progression of their solubility at the moment of their change of state.

Finally, if the cohesion of a salt had a great influence over its solution, the solvent would never be completely saturated by simple contact with it, and the solution separated from the salt, might be reduced in temperature a certain number of degrees without giving up the salt. But it is not so, setting aside the accidental circumstance of inertia of the molecules, the solution gives up salt immediately it becomes the least cooled.

Hence I am inclined to think cohesion has nothing to do in general with solution. As the elasticity of vapours, so, the solution of a body, varies with the temperature; it is doubtless, also connected with the reciprocal affinity of the solvent and the body dissolved; but the effects of affinity not being variable with the temperature, while those of solution depend essentially upon it, it would be difficult not to admit that in solution as in evaporation, the product is essentially limited to each degree of temperature, by the number of molecules able to exist in a given portion of the solution; they separate themselves for the same reason as the elastic molecules are precipitated by a decrease of temperature; and probably also, like these latter, by the compression and reduction of volume of the solvent.

Thus when the temperature decreases in a solvent saturated with a body, the molecules in excess with regard to the new temperature will be precipitated, not by virtue of the cohesion, which we suppose ought to incline them to separate and aggregate, but because they can no longer be maintained in the solvent as takes place for a vapour saturated space which has just been cooled, hence it would be of but little matter whether the molecules repulsed from the midst of a solvent,

once separated, took the solid, or liquid, or even the elastic form.

Whenever solution is essentially connected with vaporisation in this manner, that both are dependent upon the temperature and obedient to its variations. Hence they ought both to afford, if not a complete identity of effects at least a great analogy: their essential difference consists in the gaseous molecules not having need of a solvent to keep them in a given space their repulsive force being sufficient for this purpose. On the contrary in the solution of a solid or liquid body, the molecules could not keep themselves in the space if they were connected by affinity with the molecules of the solvent. This condition fulfilled, the solution following its particular course, yielding to temperature as every vapour has also one particular to itself.

Hence the analogy which solution and vaporisation have holds to their complete submission to temperature; and as the variates it appears to me incontestable, that the elastic force of the vapour of a body is quite independent of the state of this body or of the cohesion of its molecules since it remains constant when the latter varies, I shall still be disposed to admit from these analogies that solution is regularly independent of cohesion.

However, if there exist analogy between vaporisation and solution, we may ask, why while the elastic force of vapours follows a regular ascending law, the solubility of some salts such as sulphate, seleniate of soda, presents all at once a point of repulsion and a decreasing course.

I shall remark first that the difficulty remains the same whether there be an analogy between vaporisation and solution or not, and thus it cannot constitute a serious objection; in the second place the retrograding point in the solution of some bodies, may be easily explained by the consideration that at this point it is no longer the same body which continues to be dissolved. Thus for chlorine from 0° to about 8° a space of temperature during which it is in a hydrate state, the solubility is ascending but at this latter point, the hydrate is overcome and immediately, as the solubility follows a decreasing progression as far as 100 , at which it is almost nothing. This is very evidently hydrate of chlorine which is dissolved from 0° to 8° above that of chlorine only. Finally for sulphate of soda, the decrease of the solubility in proportion as the temperature increases above 33° may be attributed to a diminution of affinity. I shall return to the solubility of this salt.

As there is some interest to know whether a salt susceptible of forming a hydrate, dissolves in water, hydrated or anhy-

drated, I shall mention a fact which seems to me necessary to remove the uncertainty : it is that, whenever an anhydrous salt, or any other body not having the property of forming a hydrate, is dissolved in water, there is constantly a production of cold ; and that, on the contrary, when the salt can form a hydrate, there is a production of heat. When the hydrate is complete, before the solution in water the case is the same as when the salt cannot be hydrated. We may perceive that it might sometimes happen that the heat produced by the hydration was less than the cold produced by the change of state, but I have not yet perceived any exception. The fact that I have just particularised will also establish a fresh analogy between solution and vaporisation, relatively to the heat rendered latent in the change of state.

In comparing solution with combination, we may assign a remarkable difference between them, viz. that solution varies at every instant with the temperature while combination is not similarly obedient to these variations.

If my observations be correct, they will greatly weaken the influence which Berthollet has attributed to cohesion in all chemical phenomena ; but I feel too much the weight of this illustrious authority not to be, in defiance of my own arguments, and not to be staggered in my new convictions. It is with this sincere feeling of doubt, that I shall indicate some applications of the light in which I consider cohesion.

Berthollet has often repeated that when one body precipitates another from it, it is not always an indication of a superiority of affinity ; that it is the cohesion which takes the precipitated which determined the decomposition.

From the principles which I have established, cohesion on the contrary, has only a secondary place in the precipitation, as in the solution : the precipitation is a constant proof of a greater affinity ; cohesion only shows it by rendering its effects sensible.

With regard to decompositions by double affinity, our explanations are equally divergent. If we submit a solution of sulphate of soda with one of nitrate of lime it makes a precipitate of sulphate of lime, and nitrate of soda remains in solution.

Bergman explains this result by saying that the sum of the *active* affinities which are in motion carries it over that of the *quiescent* affinities.

According to Berthollet there is a double decomposition, because the sulphate of lime is the most coherent of the four salts which may be conceived from the mixture in the solution,

previously to all precipitation. Berthollet conceived that although the sulphate of lime does not exist, still the cohesion which it must take determines the formation of it as well as the separation.

This explanation I believe has never appeared satisfactory. As long as the sulphate of lime is expected not to exist in the solution, the cohesion that it should take cannot be cited to explain its formation and precipitation; and for the same reasons we can no more invoke the insolubility: it does not determine the change as a first cause, it only renders it sensible and effective, when it has been used in determining the separation of its products. What then is really the cause which presides over the decompositions by double affinity?

If we turn our attention to the precipitates resulting from the action of the double affinities, we find that those are not the most stable precipitates which contain acids and the most powerful bases. Thus sulphate of potassa although formed of elements endowed with a powerful affinity, is transformed in its mixture with acetate of lime, into sulphate of lime, the base of which has a much less affinity for the sulphuric acid, than the potassa. In the mixture of sulphate of lime with carbonate of ammonia, the lime is precipitated with the carbonic acid in a much less stable combination than it formed at first. It would be easy to give many similar examples.

Hence it will not be correct to say, that after the mixture of two saline solutions, the strongest acid always combines with the strongest base; it would appear on the contrary, that the salts in a state of neutralization, may change acids and bases independently of their reciprocal affinities.

Judging only by the results of experiment, the change is manifested by the precipitation of a new insoluble salt alone, whose formation, according to Berthollet, would even be the cause of the change. But as the reasons he has given for it are not satisfactory, we may ask, if the cohesion of a salt not yet existing, or its insolubility, which does not even carry the idea of cohesion, can exercise their action before the formation of this salt and be the real cause of it; or rather even, if not being able to determine this formation they only exercise their influence afterwards, causing the separation of one of the new salts produced at the moment of the mixture.

To myself, after the observations I have presented on the slight influence of cohesion in solutions and chemical precipitations, the question does not appear doubtful.

I shall recall to mind first that the solubility of a solid body in a solvent, is no way affected by the difference of molecular

attraction between the solid and liquid state, that consequently the change cannot be affected any longer.

But to these considerations we may add others which appear to me of great weight.

The change between the acids and bases of two salts may take place, according to Berthollet, in several ways. Besides the insolubility which most usually determines it, a difference of fusibility, density, and volatility, may also, very well produce it. But in the case, for example, of a difference of volatility, we can no longer invoke the reciprocal affinity of the molecules as for a solid, or even for a liquid, since, on the contrary, the molecules of the salt which is separated are in a state of repulsion, and that we may also demonstrate, as in the case of insolubility, as in that of volatility, it is always the most volatile salt that is formed.

Thus the change taking place, according to the received opinion, under very different circumstances of solubility, density, fusibility, and volatility, one of them cannot be the true cause of the change to the exclusion of the others, and consequently this cause ought to be considered otherwise, independent of these different circumstances.

Since the change is not determined by the reciprocal affinity of the acids and bases, since also it is no longer by the secondary causes we have just enumerated, and as however these latter cause separations, it necessarily follows that the change precedes them, and we can only be satisfied with regard to these different causes of separation, by admitting that at the moment of the mixture, before any separation, there is a complete confusion between the acids and bases, that is to say, the acids combine indifferently with the bases and reciprocally; the order of combination is of little importance, provided the acidity and alkalinity are satisfactory, and they are evidently so, whatever equilibrium may be established between the acids and bases.

This principle of indifference of equilibrium (*equipollence*) being established, the decompositions produced by double affinity are explained with very great simplicity. At the moment of the mixture of two salts, two new ones are formed, bearing some relationship to the two former, and allowing one of these properties, insolubility, density, fusibility, volatility, &c. to be stronger in the new than in the given salts, there will be a disturbance of the equilibrium, and separation of one salt, sometimes even of several.

Still it is essential to consider that, although we admit a confusion at the time of the mixture of two or more saline solutions, it may not always rigorously take place. We know in

fact, that the molecules of a compound oppose a sort of inertia to the change, and that time or disturbance is often necessary to cause this change. Many saline solutions, and particularly that of sulphate of soda, keep themselves sur-saturated at very inferior temperatures to that at which they ought to begin giving up the salt. A solution of sulphate of magnesia mixed with a solution of oxalate of ammonia, when left undisturbed, gives no precipitate of oxalate of magnesia for a long time after the mixture, whereas it is produced in a few seconds by means of a rapid agitation. Besides this circumstance of the inertia of molecules, which is opposed to the change, we may admit, in the case of a complete reciprocal saturation, such a state of indifference, or if we prefer it, instability between the acids and bases, that the slightest circumstance, even a very feeble cohesion, might disturb the equilibrium and determine the change.

Even admitting that the confusion has taken place, we might yet conceive that the separation of the newly formed salts, would not be instantly effected, and for this reason we see that water remains liquid several degrees below zero. Hence it is possible to conceive that the reciprocal action of the molecules which separate themselves from the solvent, determines and accelerates the phenomenon. But this reciprocal action of the molecules to reunite into a liquid or solid mass, I always consider as only occupying a secondary place in chemical phenomena.

It is easy to demonstrate the change between the elements of two salts, although it may not be accompanied by the formation of a precipitate. Let us imagine a solution of sulphate of protoxide of iron, and pass through the mixture a current of sulphurated hydrogen: there will immediately be made a precipitate of sulphuret of iron, which makes us suppose that it was formed previously from the acetate of iron. I know that in the real case, we may object to this change having taken place between the strongest acid, sulphuric, joined to the strongest base which is here the soda; but the objection will not appear founded if we recollect that the reciprocal affinity of the acids and bases appears quite foreign to the formation of precipitates formed by the concurrence of double affinities.—Every other base besides soda, the weakest we can choose among those which are not precipitated by the sulphurated hydrogen, would produce a similar effect. Thus acetate of alumine mixed with sulphate of iron, determines its decomposition by sulphurated hydrogen.

The principle of chemical equilibriums (*equipollences*) which has just been admitted with regard to saline substances, ap-

pears to me to extend to all analogous compounds, that is to say to all those in which the sum of the neutralizations, after the mixture will be the same as before, as for example, for water and a chloride.

Here is a very remarkable fact. It would appear that, in the reciprocal combination of two acids with two bases, there is expended a certain quantity of action, whether chemical or electrical, which remains constant in the change.

I had wished to say a few words on solution: but I find myself prevented by the difficulty of the subject, which is much greater than it appeared at first sight. I shall confine myself to remarking, that the word solution is applied under very dissimilar circumstances, which ought, however, to be carefully distinguished. In a solution properly so called, such as a salt in water, there is no decomposition between the solvent and the body dissolved; the effect varies in general with the temperature. On the contrary, in a solution by an acid or alkaline solvent, there is generally decomposition, formation of new products, and the effect no longer varies with the temperature as in the other solution. Hence we must determine in each particular case, whether it is simply solution, whether it is the consequence of the formation of new products, or if in that these two circumstances cannot be joined together. But to arrive at this determination some data which will form the subject of another memoir, are still wanting.

I terminate this first work without having nearly exhausted the matter it embraces, but as I said at the beginning, the matter is difficult, and I had only promised making a few observations. Perhaps they will have more interest in being strengthened by these I have yet to present. In the mean time I leave that to the criticism of chemists, and shall consider myself happy if, at least as *conjectures*, they attract their attention.

XLV. OPTICS.—*Note on irradiation; by M. J. PLATEAU.**

At the session of the 6th of May last, M. Arago much wished to entertain the Academy with my memoir on irradiation, and presented at the same time some observations on the theoretic part of this work. M. Arago thought that we could not preserve the physiological explanation which I have en-

* From the *Compte Rendue*. Translated by J. H. Lang, Esq.
VOL. IV.—No. 22, *March*, 1840. B B

deavoured to confirm, and advances a new theory from which the irradiation would be the result of the chromatic aberration of the eye. The considerations mentioned by M. Arago, not having been printed, I have been prevented from becoming perfectly acquainted with them, and am not aware of their tending to refute the arguments I have brought forward in favor of the physiological theory. I shall not here recall these circumstances, but shall content myself with examining the new hypothesis presented by M. Arago.

It is true, the eye is not, at present, recognised as a perfectly achromatic instrument, and it necessarily follows from this non-achromatism, that the images of objects are surrounded, on the retina, by a small band of aberration, which ought to increase a little the apparent dimensions of luminous objects projected on an obscure ground, and diminish those obscure objects which are projected on a luminous ground. But whether this effect can be sensible under ordinary circumstances, and whether the small band of aberration has sufficient breadth for us to distinguish it, and to attribute to it the phenomenon known as irradiation, is the question I hope to solve.

I shall first remark, that by virtue of the same cause which produces it, the small band, which the chromatic aberration of the eye draws around images, cannot be exempt from colors. Consequently, if this irradiation, manifested by a white object on a black ground, was due to this cause, it appears that the object would appear colored on the edges.—But among all the observers who have engaged themselves with ocular irradiation, not one has made the least mention of colored appearances, and in the numerous experiments that I have made on irradiation under a great number of different circumstances, I have never seen any thing similar.—This absence of visible colors, might, with difficulty, be attributed to the small angular width of the irradiation; the persons among whom the phenomenon has much development, will be easily convinced, by repeating some of my experiments, or by observing the well known appearance of the current, that the band of irradiation is of a width quite sufficient to allow its color to be seen if it had any.

In the second place, I do not see how it would be possible to explain by the aberration of refrangibility, this singular law to which irradiation is subject, viz.:—that when two objects of equal brilliancy are only separated by a small interval, each of them diminishes the irradiation of the other in the parts in view, and that, in proportion as the two objects approach one another, so that at last when they touch, the

irradiation is nothing for each of them at the point of contact; or how to admit an action exercised by a luminous image on the aberration produced about another image.

But we may easily decide by direct experiments, whether ocular irradiation is due to chromatic observation or not. It is sufficient, in fact, to try if the irradiation be also produced when the object is bright, by a homogeneous light. If in this case, we no longer perceive the irradiation, we may admit as true, the hypothesis which attributes this phenomenon to the chromatic aberration of the eye, but if, on the contrary, the irradiation still appears, and to the same degree as with a compound light, equal in brilliancy to the homogeneous light employed, it will be impossible to discover the cause of the phenomenon in the aberration with which it acts. The following experiments have been executed for the purpose of deciding this point.

The homogeneous light I have made use of is that which proceeds from the flame of a mixture of alcohol, water, and salt. I have imbibed with this mixture a quantity of cotton wick which I put behind an unpolished glass placed vertically. This mixture lighted in a dark room, gives a voluminous flame, and the unpolished glass obscured on the other side, forms a tolerably bright luminous field. To render the light still more homogeneous, I placed between the flame and the unpolished glass, a glass of a deep yellow color. Every thing being thus prepared, I placed successively before the unpolished glass, the apparatus already described in the 28th section of my memoirs, and that which served for my experiments or measure, after having reduced, in this latter, the vertical edge of the movable plate, by prolonging that of the fixed one. These apparatus thus projected, were placed on a field of very considerable brightness, and a light so nearly approached to homogeneousness, that, in observing them by refraction across a prism placed vertically at five metres distant, their image not only preserved a perfect plainness, but only presented laterally a greenish shadow so faint that it requires great attention to perceive it. I ought also to mention, that in order to give the eyes more sensibility, the experiments have not been made by day in a dark room, but at night.

But, under the circumstances I have just described, and which necessarily exclude the effects which might have depended on the aberration of refrangibility, the above apparatus have shewn me a very distinct irradiation. The same result was discovered by M. M. Burggrarve and Le Francois, two of the persons who assisted me in the experiment or measure

mentioned in my memoir, and who are, therefore, accustomed to judge of the phenomena of irradiation. In order afterwards to test the results produced, and those which would arise from a compound light, and one of a similar brilliancy, I placed before the above mentioned unpolished glass, another similar glass, behind which I lighted several wax candles so disposed as to afford an uniform light, and these I moved to and fro till the brilliancy of this second glass appeared equal to the first. An opaque screen also separated the wax-candles from the alcohol flame, so that each of the glasses received but one of the two lights. I had thus two luminous fields of the same brilliancy, but, of which, one was lighted by a homogeneous yellow light, and the other by a light, which, without being as white as that of the day, was evidently sufficiently compounded for the purpose. I then placed before these two luminous lights, the apparatus of irradiation in themselves identical, and so disposed, that in observing them simultaneously, it was easy to perceive if the irradiations developed by the two lights, differed sensibly from each other. But this comparison, made by the two persons before mentioned, and myself, gave us no appreciable difference; the two apparatus shewed a distinct irradiation, but that which proceeded from this compound light was neither more nor less extensive than that which arose from the homogeneous light.

These facts I think, lead to the following necessary conclusions, that we must admit the existence of the aberration of refrangibility of the eye; which irradiation ought to be attributed to another cause, and the effect of the aberration considered as entirely hidden, under ordinary circumstances, by the band of irradiation.

XLVI. *Brief notice of the Extrication of Barium, Strontium, and Calcium, by exposure of their chlorides to a powerful voltaic circuit, in contact with mercury as a "cathode;" and the distillation of the resulting amalgams by means of vessels of iron.**

Agreeably to the statements made by Sir Humphrey Davy in his Bakerian lecture, that celebrated chemist was not quite successful in isolating either barium or strontium, as he declares that he was not enabled to expel from them completely the mercury, by amalgamation with which they had been reduced to the metallic state from that of oxide. In the most successful experiments made by him for the isolation of calcium, the tube broke, and the mass took fire before the distillation was accomplished.

* From Silliman's Journal.

Dr. Hare has recently obtained, by an improved process, all three of the metals above mentioned. In this, saturated solutions of the chlorides are substituted for moistened oxides; the mercury and solutions being both refrigerated by ice-water, or a freezing mixture within receptacles contrived for the purpose. Two deflagrators, each comprising one hundred Cruickshank pairs, severally exposing one hundred inches of zinc surface, were employed alternately. In consequence of this mode of operating, the charge of acid, at first feeble, was gradually strengthened by additions, so as to render the reaction towards the close as forcible as at the commencement. This is highly important, since the difficulty of decomposing the chloride increases with the quantity of calcium combined with the mercury.

The resulting amalgams were severally subjected to distillation by means of a crucible enclosed in an air-tight iron alembic, being protected from the access of air by caoutchoucine naphtha, mercury and desiccated hydrogen. For the complete expulsion of the mercury, a heat above the softening point of glass was necessary.

So great was the avidity for oxygen of the metals thus obtained, that to see their bright, metallic white colour, the eye must follow closely after the movements of a file or burnisher employed to expose a fresh surface. Metallic whiteness is soon succeeded by a straw color, as in the case of steel filed at a high temperature. But the whole mass is soon reduced to a pulverulent oxide. Of this the color is dark, in consequence of a resinous coating resulting from reaction of the metal with the naphtha necessarily employed to prevent the excess of atmospheric oxygen. In consequence of this coating being insoluble in water, but readily soluble in hydric ether, oxidizement ensues more readily in the last-mentioned liquid than in water.

The metals in question were all brittle, and much harder than potassium or sodium. By the evolution of the mercury, they are left in a form resembling, in some degree, that of metallic arsenic.

Davy informs us that he employed only fifty or sixty grains of mercury. Dr. Hare has employed a half-pound avoirdupoise, which is seventy times as great, and is under the impression, that with sixty grains it would not be possible to isolate a perceptible quantity of calcium. Operating with much larger quantities of amalgam, he has found no residue besides a stain upon the glass of the tube employed to distil off the mercury.

XLVII. *Process for a Fulminating Powder—for the Evolution of Calcium and Galvanic Ignition of Gunpowder ; by DR. HARE.*

An equivalent of quick lime, with an equivalent and a half of bycyanide of mercury, is subjected to a red heat in a porcelain crucible enclosed within an air-tight alembic of iron, so as completely to exclude atmospheric air. The resulting residual mass was found, in two experiments, to have the weight which would correspond with an equivalent of calcium, united to an equivalent of cyanogen. From the filtered solution of the compound thus produced, in acetic acid, a precipitate was obtained by the addition of nitrate of the protoxide of mercury. This precipitate when well dried was found to constitute a powder capable of fulminating by percussion.

Isolation of Calcium by the deflagration, in a receiver, of desiccated hydrogen, of the compound formed by igniting in a close vessel, bicyanide of mercury with pure quick lime.

By exposing the compound of cyanogen with calcium, obtained as above mentioned, either in vacuo or in an atmosphere of desiccated hydrogen to a current from two hundred pairs of Cruickshank plates, each comprising one hundred square inches of zinc surface, the calcium appeared to be isolated. Particles displaying metallic characteristics under the burinisher, and which effervesced in water, were observed, while the gas escaping had an odor resembling that of silicuretted hydrogen evolved by silicuret of potassium, under like circumstances.

Deflagration of phosphuret of calcium.—By exposure of the phosphuret of calcium to the current from the deflagrators, as above described, calcium containing a trace of phosphorus appeared to remain. The phosphorus was condensed upon the receiver in sufficient quantity to obscure the glass. The residual mass thrown into water effervesced extricating hydrogen slightly phosphoric in its odor. When compounds of carbon with calcium were similarly exposed, the residue had a metallic appearance, but did not decompose water.

On one occasion, a portion of the charcoal forming the anode was fused into a globule, having the consistency and other characteristics of plumbago. It appeared more compact than the globules obtained by us many years since, of which a portion was forwarded to Dr. Hare at the time.

Of Professor Daniell's adoption of Dr. Hare's method of igniting gunpowder by galvanic ignition.

During the summer of 1831, a method of igniting gunpowder by galvanism was contrived by Dr. Hare, the idea having been suggested by the abortive efforts of an ingenious individual of the name of Shaw, to effect this object by mechanical electricity. Of the apparatus described for the purpose in question by Dr. Hare, engravings and descriptions were published in this Journal in the autumn of 1833. We advert to these facts now in consequence of the recent publication of analagous experiments by Pro. Daniell, King's College, who in this case, as well as in that of his "re-invention" of a hydro-oxygen blow-pipe of Dr. Hare, was no doubt ignorant that he had been anticipated.

In performing his experiments, it would seem that Pro. Daniell used his ingenious apparatus, known as the sustaining battery, which, although peculiarly qualified for the production of a durable current, is, as we think, far less competent than the calorimotor of Dr. Hare, to produce a transient intense ignition, such as would be the most efficacious in igniting gunpowder.

XLVIII. *Method of adjusting the Dipping Needle ;* by
THOMAS PERRY, Professor of Mathematics, United States
Navy.*

TO THE EDITORS.

Gentlemen,—Finding it necessary, some months since, to re-adjust a needle belonging to my instrument for measuring the magnetic dip, I adopted a method which, from its simplicity, I am induced to communicate, in the hope that it may be serviceable to others in similar circumstances.

The instrument being firmly fixed, and accurately levelled, the direction of the magnetic parallel of latitude and meridian, and the true dip, were approximately ascertained by properly reversing its faces, axis, and poles. The plane of its face was then made to coincide with the parallel of magnetic latitude, and the substance of the needle carefully ground away, *from the sides perpendicular* to its plane of motion, until it assumed the same position (the vertical) upon reversing its axis. The plane of the face was then brought into the magnetic meridian, and the needle again ground upon the sides parallel to the plane of motion, so as not to affect the previous adjustment much, until it indicated nearly the true dip. These processes were successively repeated, until the errors,

* *Silliman's Journal.*

saving such as result from the imperfection of the circles were found, upon making all possible reversions, to be less than the probable errors of observation.

This method may be advantageously employed in the final adjustment of new needles. I have employed it successfully in one instance. Two small screws, at right angles with each other, might also be added, which would render grinding unnecessary; but their weight would prove some incumbrance, and they would increase the liability of the adjustments to derangement.

The value of the process results from the difficulty of rendering manufactured and tempered steel devoid of magnetism. Its correctness of principle is obvious from the impossibility of correct indications in two different positions of the needle, except when the centre of gravity coinciding with the axis of motion, the influence of this force becomes nothing in all cases.

In making these adjustments, it is better that the magnetism be of feeble intensity, provided that it be sufficient to overcome inertia and friction; as, in this case, the influence of any other force is more obvious. Any two different planes or even the same might be employed by a little modification of the process; but those specified are most eligible, as in them the forces affecting the position of the needle, present the greatest disparity.

U. S. Ship Independence, Jan. 28, 1839.

XLIX. *Formula for discovering the Weight and Volume in a mixture of two Bases, by Dr. JNO. M. B. HARDEN, Riceboro, Liberty County, Geo.**

TO THE EDITORS.

In the 12th volume of the "Philosophical Magazine" there is a paper by Mr. Golding Bird, upon the subject of "indirect chemical analysis," in which he gives two formulæ, by Puggendorff, for the quantitative estimation of two different bases in mixtures of those bases. These formulæ are sufficiently exact, but probably not as simple or comprehensive as might be desired. He alludes also to one annexed by the French translator to the "Analysis of inorganic bodies," by Berzelius, which I do not find in the English translation of that work. As it may be well to multiply methods for the solution of such problems, I send you the following formula, which, although from the well known principles which it involves, I cannot suppose that it has any claim to novelty, I

• Silliman's Journal.

have never seen proposed for this object. If you should consider it worthy the notice of the analytic chemist, you will please insert it in your highly useful Journal.

In the mixture of two bases, it is proposed to find the weight and volume, or bulk of each base, by having given the specific gravity of each ingredient, together with the specific gravity of the mixture and its weight. Now, since the specific gravities of each base or ingredient of the mixture are supposed to be known in most, if not all cases, all that is necessary will be to determine by experiments, the specific gravity and weight of the mixture, in order to find the quantities desired. Let A = sp. gr. of one ingredient, B = sp. gr. of the other, and C = sp. gr. of mixture. Let also the weight of the mixture = 1, and x and y = the weights of the bases; then it is evident that

$$\frac{x}{A} + \frac{y}{B} = \frac{x+y}{C} = \frac{1}{C} \text{ and } x + y = 1.$$

These equations reduced, give

$$x = \frac{AC - AB}{AC - BC} \text{ and } y = \frac{AB - BC}{AC - BC}.$$

Multiply these fractions by the number expressing the weight of the mixture, and we have the weight of each base or ingredient; and as the volumes are inversely as the specific gravities, they are found by dividing the weights by the sp. gr of each.

We give as an example, the mixture of oxygen and azote in atmospheric air

$$x = \frac{1.1111 - 1.1111 \times .9722}{1.1111 - .9722} = \frac{309}{1389} \text{ proportional weight of oxygen.}$$

$$y = \frac{1.1111 \times .9722 - .9722}{1.1111 - .9722} = \frac{1080}{1389} \text{ do. do. of azote.}$$

Now, since 100 cubic inches of air weigh 30.5 grains, it will be found that the *weight* per cent. of oxygen in atmospheric air is 22.23, and of azote 77.77, divide these by the sp. gr. of each, and it will be found that the *volume* per cent. of azote is 79.8, that of oxygen 20.2 nearly, which corresponds exactly with the result of the most rigid and careful experiments.

I need scarcely remark that this formula applies only in cases where the specific gravities are determined by the same standard of comparison, although in every case they may be reduced to the same by an easy mathematical calculation.

Liberty Co. Geo. Aug. 15th, 1839.

L. *Of the Reaction of Sulphuric Acid with the Essential of Hemlock*; by Mr. CLARK HARE, of Philadelphia.*

If equal parts of sulphuric acid and oil of hemlock be mingled together, refrigeration being employed to prevent too great a rise of temperature, a black acid resinous mass results. By the addition of carbonate of lead and water, the unaltered sulphuric acid, present in great quantity, is converted into an insoluble sulphate, which, mingling with the resin, gives rise to a yellow mass resembling putty in its consistency, while there will be found dissolved in the water two soluble salts of lead.

The presence of a very large quantity of coloring matter, interferes with the examination of these salts. This, however, in a great measure disappears on precipitating the lead by sulphydric acid gas, resaturating the liberated acids by the carbonate, and again throwing down the lead in the state of a sulphide. The partially decolorized acids thus obtained may then be saturated with barytes, and the resulting salts evaporated to dryness, when they assume the appearance of an amorphous mass. By washing with absolute alcohol, one of the salts present in this mass is dissolved. On the solution of the other in water, and subsequent crystallization, it proves to be the acetate of barytes.

The salt dissolved in the alcohol does not appear susceptible of crystallization, probably on account of its extreme solubility. On drying it assumes a gummy appearance, and by still farther desiccation, may be obtained in the state of a dry mass destitute of cohesion, and susceptible of being with facility reduced to the state of a powder.

When exposed to heat in a retort, this salt resists an elevated temperature without alteration, but at length, if heated rapidly, carbonizes, giving off sulphurous acid and a small quantity of essential oil and water. There remain in the retort a spongy carbonaceous substance, and a large quantity of sulphite of barytes. As this result proved the acid united with the barytes to consist of organic matter, combined with sulphuric acid and

modifying its properties, in order to ascertain the quantity of the latter present, barytes was precipitated by carbonate of potash, the precipitate weighed and the resulting potash salt evaporated to dryness. It was then intimately mingled with the black oxide of copper and nitrate of potash, nitric acid added, and the whole mass gradually heated to redness. Red fumes are given off during the whole of the process, and while the nitric acid at the beginning of the operation prevents the deoxidation of any portion of the sulphuric acid; at the end, the oxide of copper prevents the explosive reaction which would ensue, were nitric acid and nitrate of potash alone present.

The result of two experiments made in this manner, the mass after ignition being washed with diluted chlorohydric acid, and the solution precipitated by barytes, was as follows:—Carbonate of barytes $12\frac{1}{2}$ gr's. Sulphate of barytes $16\frac{1}{2}$ gr's. Carbonate of barytes $13\frac{1}{2}$ gr's. Sulphate of barytes $16\frac{1}{2}$ gr's. The quantity of sulphuric acid as calculated from the quantity of sulphate precipitated, is in each case, 5.59 gr's., while as calculated from the precipitate of carbonate of barytes, on the supposition that one atom of it is present in the barytes salt for each atom of base, it would be 5 gr's. in the first instance, and 5.3 gr's in the second. It will therefore be perceived that in both experiments the quantity of sulphuric acid, as calculated from the results, exceeds the quantity necessary for forming an equivalent with the base present. This must be attributed either to some inaccuracy in performing the analysis, or to the presence of a small quantity of some sulpho-organic acid, containing in its neutral salts, two atoms of sulphuric acid for each atom of base. The former explanation is by far the most likely to be true, and it seems probable that the composition of a neutral salt of this acid may be represented by one atom of sulphuric acid, one atom of organic matter, and one atom of base.

A number of compounds possessing the properties of acids have been discovered, consisting of an acid of sulphur modified by some organic substance. These compounds may be divided into two classes. In one are comprised those acids which are composed of two atoms of sulphuric acid, united to one of organic matter acting as a base, and which consequently, in forming neutral salts, unite with but one additional atom of base. In the neutral salts formed by the other class, two atoms of sulphur are also present for each atom of organic matter and each atom of base, but are combined with oxygen in such proportion as to form hyposulphuric acid, so that the organic matter present cannot be considered as acting the part of a base. Under the first of these heads may be enumerated the

sulphovinic, sulphetheric, sulphomethylic, and sulphocetic acids; under the second, the benzosulphuric, sulphonaphthalic, and probably the sulphovegetic, and several others. For the acids contained in the first class, custom seems to have assigned as a nomenclature, a name derived from the organic matter entering into their composition, modified so as to terminate in *ic* and having the term *sulpho* prefixed. For the second, no fixed rule seems to have been laid down. The German chemist who discovered one of the two acids whose composition has been ascertained with sufficient accuracy to enable us with certainty to place them under this head, gave to it the name of benzosulphuric, while the other acid still retains the appellation of sulphonaphthalic, which it received when its composition and properties were still supposed to be analogous to those of the sulphovinic and other acids which belong to the first class. The acids described in this article, if the view given of its composition be correct, must be considered as belonging to a division of the second class hitherto unoccupied, unless by the sulphindigotic acid of Berzelius. In the hemlosulphuric, as in the other acids of this class, there is present one atom of an oxacid of sulphur modified by an atom of organic matter which does not, as in the first class, act as a base, or diminish the saturating power of the acid. If, therefore, we should adopt the nomenclature of the German chemists, with the change of sulphuric into hyposulphuric as necessary to designate with precision the acid of sulphur in question, for the acids of the second class, calling them benzohyposulphuric and naphthalohyposulphuric; and applying the same idea to the acid described in this article, name it hemlohyposulphuric, the ends to be attained in forming a nomenclature would perhaps be as well answered as is practicable, without departing too widely from established custom.

Hemlosulphuric acid possesses a sour taste and peculiar odor. It does not appear susceptible of crystallization, either when free or as far as I have examined its compounds, when combined with bases. The salts which it forms with potash lime and barytes leave in the mouth a decided and long continued impression of sweetness. Though extremely soluble they are not deliquescent. If the hemlosulphate of barytes be kept for a length of time at a temperature between 500° and 600°, the sulphate of barytes and organic matter of which it is composed separate, the latter in the shape of a resinous powder insoluble in water, though soluble in alcohol and ether. This seems a singular instance of a body very soluble in water, affording by the mere separation of its constituents, two others eminently insoluble in that liquid.

In the resinous yellow mass into which the greater part of the hemlock of oil is converted by the action of the sulphuric acid, there is present a yellow oil which contains sulphuric acid combined with it in a neutralized state. By the action of ether, this oil may be dissolved, and by subsequent evaporation, deposited, but when thus obtained it is contaminated by so much resin that though the presence of sulphuric acid may be ascertained, it is impossible to determine the atomic composition.*

From the reaction of sulphuric acid, with oil of turpentine, nothing more appears to be produced than a reciprocal decomposition; though a different result might have been anticipated from the close analogy which appears to exist between this essential oil and that of hemlock. Caoutchoucine, however, reacts with sulphuric acid in a manner quite analogous to the oil of hemlock, giving rise to a yellow resin and an acid compound of sulphuric acid and organic matter, which forms soluble salts with lead and barytes. An oil, however, separates and floats on top, which appears insusceptible of farther attack from the acid.

LI. Results of Experiments on the Vibrations of Pendulums, with different suspending springs; being the substance a paper by W. J. FRODSHAM, F. R. S., read before the Royal Society, June 21, 1838. Forwarded for insertion in this Journal.

The experiments of which I am about to give an account, and from which I propose to draw some practical conclusions, were undertaken with a view to determine whether some particular condition of the suspending spring of the pendulum, with respect either to its length, its strength, or both, might not cause it, with a lighter maintaining power to produce a given arc of vibration, or, with a given maintaining power, to produce a greater arc of vibration than any other; and at the same time to ascertain whether some practical means might not be devised for making unequal arcs of vibration in the ordinary pendulum, correspond to equal intervals of time.

My attention was drawn to the subject many years ago, when having replaced the spring of a turret-clock by a stronger one, I found the arc of vibration materially altered.

* It is well known that by the reaction between chouchydric acid and pure oil of turpentine, two species of artificial camphor are generated, one solid, the other liquid. Having obtained both of these compounds a few years since, Dr. Hare subjected the oil of hemlock to chlorohydric acid by the same process, but could not thus obtain any concrete camphor. That which he did obtain was analogous to the liquid artificial camphor above mentioned.

Having often reflected upon the subject, I at length resolved to make some experiments to satisfy my mind respecting it; and I accordingly had made for the purpose a lenticular pendulum bob of about fourteen pounds weight, a cylindrical rod passing through it, with a nut working on a screw at the lower end, and supporting the bob.

The upper end of the rod was slit to receive the spring; and the spring and the rod were attached to each other by a pin passing through a hole in both.

But before fixing the pin, what I call an *isochronal piece* was slid over the top of the rod, and if this part of the apparatus had served only to attach the rod and spring more firmly together, and prevent any wavering motion of the pendulum, it would have rendered an important service. This, however, was but a secondary and incidental effect of its application.

The piece, which I have so named, is a brass tube about five inches long, fitting the pendulum rod very nicely, and slit to form a spring for about an inch at the bottom, so as to slide rather stiffly on the rod. At the upper end of the tube is a *clip*, which is made to embrace the suspending spring firmly by means of two screws; so that after the pendulum has been brought to the proper length by the adjusting nut at the lower end of the rod, the length of the acting part of the suspending spring may be varied at pleasure, without in the least altering the length of the pendulum, by merely sliding the isochronal piece up or down the rod, and tightening the screws of the *clip*.

I also provided five springs of different degrees of strength, and a silken string, by which, in the first experiments, the pendulum was suspended.

The pendulum used was an uncompensated one, but in each experiment it was adjusted to nearly the proper length for mean time.

Commencing with the silken thread, or rather two parallel threads, one behind the other, I suspended the pendulum within the case of a clock, perfectly detached from the works, no maintaining power being applied.

Each degree of the scale on which the arcs of vibration were noted, was nearly $\cdot 8$ of an inch in length, and a degree was sub-divided into twenty equal parts.

I drew the bob aside 2° , and leaving it to vibrate by its own gravity, I found the arc of vibration was reduced from 2° to 1° , and from 1° to $\frac{1}{2}^\circ$, in the times noted as under.

Arc of vibration from 2° to 1° in 20m. 15s.				
Do.	do.	1 to $\frac{1}{2}$	23	6

On repeating the experiment, the results were :—

Arc of vibration from 2° to 1° in 21m. 0s.				
Do.	do.	1 to $\frac{1}{2}$	24	0

Drawing the pendulum aside 1° , I found from five successive trials that the arc of vibration was reduced to half a degree in the times following :—

From 1° to $\frac{1}{2}^{\circ}$ in 21m. 45s.				
Do.	do.	22	45	
Do.	do.	22	0	
Do.	do.	22	30	
Do.	do.	23	0	
				<hr/>
Mean,	-	-	-	22 24

The mean of the two preceding corresponding results is 23m. 12s. The difference may be satisfactorily accounted for, by the difficulty of setting off the pendulum at the precise point intended, and of noting the time when the arc is diminished to the proposed quantity.

It is apparent from these experiments, that when a pendulum is freely suspended, and left to vibrate from its own gravity, the arc of vibration is sooner reduced from 2° to 1° , than from 1° to $\frac{1}{2}^{\circ}$, as might indeed be anticipated from the increased resistance experienced by the bob, while moving through a greater space in the same time.

I attached the pendulum, suspended as before, to a clock, with a maintaining power of 6lb. 8oz., but the clock stopped in 39 minutes; and setting it off again, it stopped in 43 minutes; but on applying a weight of 6lb. 11oz., the clock continued to go; thus showing that a weight of 6lb. 11oz. was sufficient to keep the pendulum in vibration, while one only 8oz. lighter was not.

The arcs of vibration in the preceding experiments being smaller than is desirable in practice, I proceeded to experiment with heavier weights, the pendulum being still suspended by the parallel silk threads, noting in each case the arc of vibration and the *rate* of the clock, viz., its gain or loss in 24 hours.

In the following experiments each succeeding pair is to be considered as giving the results for two consecutive days, though more than one day occasionally elapsed between the times at which the sets were taken.

Weight.		Arc of Vibration.		Rate.	
14	6oz.	2°	3,	— 9s	.0 }
8	0	1	30	+ 0	.7 }
14lb.	6	2°	3	— 10	.0 }
9	0	1	30	0	.0 }
11	2 1	1	45	— 7	.0 }
8	0	1	30	+ 1	.0 }
= 19	0	2	15	— 13	.0 }

It hence appears, that when a pendulum is suspended by a flexible string, a heavier weight and a consequent greater arc of vibration, causes the clock to lose.

The following are the dimensions of the springs which were experimented with:—

Number.		Breadth.		Thickness..
1	-	.350 inch	-	.001 inch.
2	-	.390	-	.002
3	-	.395	-	.003
4	-	.395	-	.004
5	-	.400	-	.035

The pendulum being suspended by the weakest string, No. 1, the times were noted as before, in which the arcs of vibration were reduced from 2° to 1°, and from 1° to $\frac{1}{2}$, no maintaining power being applied.

Arc reduced from 2° to 1° in 1h. 58m.			
Do.	do.	1	57
Do.	1 to $\frac{1}{2}$	2	8
Do.	do. $\frac{1}{2}$	2	10

With the same spring, and a maintaining power of 4lb. 1oz. and 2lb. 2oz., the following arcs of vibration and rate of the clock resulted from two consecutive days, the effective length of the spring being .92 inches.

Weight.	Arc.	Rate.
4lb. 1oz.	2° 3'	— 9s. 6
2 2	1 30	— 6 1

The pendulum being suspended with spring No. 2, and clipped at .92 inch, without maintaining power, the arcs of vibration were reduced as follows:—

From 2° to 1° in 2h. 20m. 0s.			
Do.	2 — 1 — 2	20	44
Do.	1 — $\frac{1}{2}$ — 2	26	0
Do.	1 — $\frac{1}{2}$ — 2	26	0

Applying 4lb. 1oz. and 2lb. 2oz. in succession, as a maintaining power, I found as under:—

Weight.	Arc of Vibration.	Rate.
4lb. 1oz.	2° 9'	-0s. '2
2 2	1 36	+2 '5

With spring No. 3, and effective length '92 inch, the following results were obtained on two consecutive days:—

Weight.	Arc.	Rate.
4lb. 1oz.	2° 15'	—2s. '5
2 2	1 39	—2 '8

Reducing the effective length of the spring to '8 inch, the following results were obtained on consecutive days:—

Weight.	Arc.	Rate.
4lb. 1oz.	2° 9'	0s. '0 }
2 2	1 30	0 0 }
4 1	2 9	—0 '5 }
2 2	1 30	—0 '2 }
4 1	2 9	—0 '2 }

Hence, with either of these lengths of this spring, the rate does not appear to be perceptibly influenced by the extent of the arcs of vibration. In fact, the vibrations of the pendulum may, for all practical purposes, be considered as isochronous.

The effective length of the spring was then increased to '92 inch, and the following results were noted, without maintaining power:—

Arc reduced from 2° to 1° in 2h. 26m. 0s.					
Do. do.	2	1	2	25	45
Do. do.	1	$\frac{1}{2}$	2	37	0
Do. do.	1	0	2	36	40

On three other occasions, with the same spring, and effective length '92 inch, the following comparative results were obtained:—

Weight.	Arc.	Rate
4lb. 1oz.	2° 15'	—4s. '0 }
2 2	1 39	—4 '2 }
4 1	2 15	—5 '0 }
2 2	1 39	—5 '2 }
4 1	2 15	—5 '0 }
4 1	2 15	—5 '0 }

Shewing that even with different lengths of this spring, the vibration may be considered as isochronous, with considerably different arcs of vibration; and also that with this spring, a greater arc of vibration is produced with the same maintaining power, than with any other spring that has been tried.

Spring No. 4 was next applied without maintaining power.

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With it the arc of vibration was from

2° to 1° in 1h.	47m.
do.	1 48
do.	1 50
1° to $\frac{1}{2}$ °	1 54
do.	1 55
do.	1 58
do.	2 0

Applying maintaining power of 4lb. 1oz. and 2lb. 2oz. respectively, with .97 inch effective length the following results were noted :—

Weight.	Arc.	Rate.
4lb. 1oz.	2° 6'	— 2s. .2
2 2	1 30	1 2

Even with this comparatively stiff spring, the arc of vibration is greater with a maintaining power of 4lb. 1oz. than it was with 14lb. 6oz., when the pendulum was suspended by two parallel silk threads. But the rate appears to vary more with the arc of vibration, than it did when No. 3 was used.

Reducing the length of this spring to .66 inch, the following results were obtained :—

Weight.	Arc.	Rate.
4lb. 1oz.	2° 3'	— 14s. .1
2 2	1 27	— 11 .5

Sliding up the isochronal piece still further, till the length of the effective part of the spring was reduced to .50 inch, the following were the results :—

Weight.	Arc.	Rate.
4lb. 1oz.	2° 3'	— 18s. .0
2 2	1 12	— 14 .5

This further shortening of the spring appears to have had a perceptible effect on the arc of vibration, when the lighter weight was applied.

I lastly attached the strongest spring, No. 5, and with effective length 1.0 inch.

Weight.	Rate.
4lb. 13oz.	— 15s. .5
2 10	— 13 .5

Reducing the length of this spring to .8, the following results were obtained :

Weight.	Rate.
6lb. 3oz.	— 14s. .6
2 10	— 12 .4

Sliding up the isochronal piece still further, till the length of the effective part of the spring was reduced to $\cdot 50$ inch the following were the results :

Weight.		Rate.
4lb.	13oz.	— 12s. $\cdot 0$
2	10	— 8 $\cdot 2$

The lighter weight, 2lb. 2oz. employed on experimenting with the weaker springs, was found insufficient to keep the pendulum in vibration with No. 5; 2lb. 10oz. was found adequate to the purpose, and it was therefore employed.

In experimenting with this spring, the arcs of vibration were not noted, as I found that both it and No. 4 were too strong for the weight of the bob I was using, and to which the experiments indicate that No. 3 was excellently adapted.

The arc of vibration with the spring, No. 3, (viz. $2^{\circ} 15'$) using a weight of 4lb. 1oz. required 19lb. weight to produce it when the pendulum was suspended by the silken threads.

It appears then, from the preceding experiments on suspending springs differing in length and strength, that there is one which, with a given maintaining power, produces a greater arc of vibration than others, and gives the same arc of vibration with a smaller maintaining power; and, further, that with this same spring the vibrations may, in point of time, be all considered as isochronous, whether the arcs are large or small. And with the aid of the *isochronal* piece, a spring of the proper length and thickness may easily be selected in a very few trials.

It may be noticed too, that unless this pendulum is first *isochronized* by some such method as that which has been pointed out, anomalies may be imputed to *imperfect compensation*, which have their origin in a very different source.

In fine, it may be stated in conclusion, that if the pendulums of astronomical clocks were furnished with what I have called an isochronal piece, any person possessing a few springs of different degrees of strength, may with very little difficulty determine what spring is best adapted to the weight of the pendulum, and also what part of the spring may be most advantageously employed in action; and I shall not think that the attention which I have given to this subject has been mispent, if any thing that I have done may contribute to the advancement of an art to which I have been professionally devoted during the whole of my life.

London, March, 1839.

LII. *Effects of Lightning upon the packet ship New York ;*
by Mr. CHARLES RICH, at the request of the Editors.*

UPON my first visit to Liverpool in May, 1827, the vessel in which I arrived was moored in Prince's dock along side the packet ship *New York*, Capt. Bennett. This ship I repeatedly visited, and indeed was obliged to cross her deck to reach the wharf. Having been informed that she had been injured by lightning during her passage, I examined her several times, and the following are the main facts that I remember.

The ship sailed from New York in April, and on the third day out, being the 19th, while in the Gulf Stream, in lat. $38^{\circ} 9'$ N. and lon. $61^{\circ} 17'$ W., was struck by lightning at about daylight in the morning. The passengers being still in their berths, were roused by a heavy report like that of a cannon close to their ears, and the cabin was filled with a dense smoke smelling like sulphur. It had been broad daylight, but was now almost dark as night. Rain fell in torrents—hail covered the deck; the lightning and thunder were almost simultaneous; the sea ran very high, and the water being at 74° F. and the air at 48° , the copious evaporation produced pillars of condensed vapour reaching to the clouds. The scene was one of terrific sublimity. Some parts of the ship and spars were for a moment on fire, but were quickly extinguished by the rain.

The fluid first struck her main royal mast, burst asunder three stout iron hoops with which it was bound, and shattered the mast head and cap. It passed down the mainmast, one branch entered a store-room and demolished the bulk heads and fittings; thence it went into the cabin, and conducted by a lead pipe passed out through the ship's side between wind and water, starting the ends of three five inch planks. During its progress it burst open the harness casks, shivered to pieces the large looking glass in the ladies' cabin, and being conducted by the quicksilver on the back, it left the frame uninjured; it overturned the piano forte, split into several pieces the dining table, and by its influence so highly magnetized the chronometer as to render it during *that* passage not trust-worthy. Most of the watches which were under the gentlemen's pillows were so highly magnetized as to stop them, and render it necessary to remove all the steel work. The gentlemen themselves were, without exception uninjured, owing doubtless to

* With additional facts selected by the editors from the full account published in Liverpool, May 12, 1827, and quoted in the *New York Spectator*, June 20, 1827—*Silliman's Journal*.

the non-conducting properties of the beds upon which they were sleeping. At the time the ship was struck, the lightning conductor had not been put up; but it was immediately after the accident raised to the main-royal-mast head.

The conductor consisted of an iron-chain with links one fourth of an inch thick and two feet long, turned into hooks at each end; at the top it ended in an iron rod half an inch thick and four feet long, having a polished point and rising two feet above the mast head; the chain descended down over the quarter, and being pushed out from the ship's side about ten feet by an oar, descended a few feet below the surface of the water.

Near two o'clock, P. M. it was observed that only four seconds intervened between the lightning and the thunder. At two o'clock there was a simultaneous flash and a shock like that in the morning; passengers in the cabin saw the appearance of a ball of fire darting before them while the glass in the round house came rattling down. To those on deck the ship appeared to be in a blaze, so vivid was the flash which they saw distinctly darting down the conductor and agitating the water. All parts of the ship as before were filled with smoke smelling of sulphur. Although the conductor was of the size which Dr. Franklin thought sufficient to sustain the severest shock of lightning without injury, yet it was literally torn to pieces and scattered to the winds, while it saved the ship. The pointed rod at the top of the conductor being fused, was shortened several inches and covered over with a dark coating; some of the links of the chain had been snapped off and others melted.*

The shock affected the polarity of all the compasses on board, causing them to vary from the true point and to range between each other, but they gradually returned within three points of truth. The chronometer of Captain Bennett, the commander of the ship which did not usually vary more than three seconds in crossing the Atlantic, was now quite out of time; it had gained for a considerable period seven-tenths of a second (in 24 hours,) and being 9m. 42s. *slow* of Greenwich time when the vessel left New York, was found at Liverpool to be 24m. 33s. *fast* of Greenwich, making a difference of 34m. 15s.

Three gold lever watches belonging to gentlemen passengers became so magnetized as to require that the principle part of

* It is said that the same thing once happened in a Dutch church in New York; a chain connected with the clock was melted and probably saved the church.

the steel work should be removed. These parts had become true loadstones acting as magnets. It is in our recollection also that in other accounts published at the time it was stated that the knives and forks and other articles of steel and iron became magnetized. Happily no person was killed, although several were knocked down and more or less injured.

Remarks.—In consequence of receiving the notice* communicated by Mr. Rich, we have been induced to republish the principal facts in the case of the packet ship *New York*, although the events happened twelve years ago. The case was so remarkable, that the results ought to be preserved as part of the permanent records of science.

No case could more decisively prove the importance of conductors. Had the ship been furnished with the iron chain and rod at the moment of the first stroke it is almost certain that she would have escaped with little or no injury. Had the topmast which was then shivered (its stout iron bands two or three inches broad and half an inch thick being burst asunder) been protected, there can be no doubt that the lightning would have shot down the conductor, saved the mast, and passed harmlessly into the sea. This was decisively proved in the second case, when the ship was again struck at two o'clock, P. M.

Her iron chain was then up, and the pointed iron rod ascended two feet above the highest topmast. She appears to have been enveloped in a condensed electrical atmosphere; the clouds being so low that the flash and explosion were simultaneous; and had there been no conductor, the second stroke, which appears to have been more powerful than the first, might have proved fatal to many of those on board. The discharge which the conductor received seems to have been more than it was able to convey away; hence some of the people were prostrated although not killed; they were evidently affected mechanically by the explosion, and electrically by the all-pervading electrical atmosphere around, but not being made part of the chain of discharge they escaped with little harm. The conductor was melted at the top and glazed, doubtless with vitrified oxide, and the chain exploded in fragments all about the ship. This proves that the conductor, although it preserved the ship, was not perfect in construction or sufficient in size.

Hooks and chains are objectionable because the continuity of communication is interrupted by the intervening films of

* Of which a short account was published in this Journal, Vol. XXI, p. 351.

air. It were much better to adopt the rope made of twisted copper wire. It might be made of any desired size, and having perfect continuity, there would be no interruption to the passage of the electricity. Being perfectly flexible, it might easily be coiled and stowed away like any of the rigging, and it would adapt itself to any flexion of the spars and masts. It should be terminated above by a solid pointed conductor of copper or iron. Such a protection as this we can hardly doubt would prove sufficient, although in the case of very long ships it might be proper to have more than one conductor. In steam ships there is an additional protection derived from their vast metallic apparatus which by its communication with the water affords the best possible channel of discharge.

It is true that some years ago an explosion occurred in Charleston harbour, in the boiler of the Savannah steam packet, from her being struck by lightning; caused possibly by the sudden expansion of the steam already generated, or the sudden generation of more steam by the intense heat. In conversation with the late Mr. Samuel Howard in whose charge the boat was at the time, he distinctly attributed the explosion to the lightning.*

In the case of steam ships it may therefore be prudent to pass the conductor directly into the water and not to the boilers or other metallic apparatus; although we should hardly expect any mischief, especially in the Atlantic steamers, whose amount of conducting surface is so prodigious. Every thing however goes to prove that all ships, especially ships for passengers where the risk of life may be great, should be provided with the best metallic conductors.

Another fact which is remarkable in the case of the packet ship New York, is the energetic magnetism that attended the lightning; chronometers, common watches, and compass-needles being all (by the lightning) rendered erratic and dangerous guides, no longer to be relied on. We conceive that good conductors would probably prevent or greatly mitigate even these effects; but as it may not be possible entirely to shun the effects of electricity, and as it is of the utmost importance that the compass-needle should always be correct, we venture to suggest a remedy.

Let every ship be provided with a small calorimotor and the appendages of helix-wires, acids, &c. With this apparatus the needles could be instantly restored or new ones (unmag-

* He was a gentleman of uncommon intelligence and good judgment—*SEN. ED.*

netized and carried for the purpose) may be magnetized with certainty and with all requisite energy and dispatch. Practical directions can easily be given if desired.

New Haven, September 9, 1882.—Eds.

LIII. *On a remarkable property of Electrical Tension*; By CHRISTIAN DOPPLER, Professor of Mathematics at the Polytechnic Institute of Prague. *From the Zeitschrift für Physik, &c., Vol. V, Part 8, p. 342. Vienna, 1837.*

Marked and decided as has of late been, thanks to the researches of the first philosophers of the day, our progress in all the branches of electrical science, and active as their endeavors have been to add further facts to those we are already in possession of with respect to the reciprocal action of electric currents on each other or upon magnets, or, inversely, the influence of the latter on electrical currents, yet, we are nevertheless, forced to confess, that the insight which we have gained into the essential nature of this mysterious fluid, has by no means kept pace with our progress in other respects.

And however remote our hope may be of seeing this portion of natural science worked out in the satisfactory manner that others have been, yet we cannot but assent to the importance, and indeed the ultimate necessity, of entering upon the enquiry: and consequently, every effort we make and every fact we can adduce, tending, even indirectly, to further the investigation, is worthy of attention.

Bearing this in mind, I do not hesitate to make known the results of an experiment, which, should its truth be borne out by subsequent observers, may possibly lead to inferences of some importance.

Some years ago, on the occasion of my publishing an essay upon the kindred subject of the probable causes of electrical excitation,* I was led to the conclusion, that wherever there is a case of electrical tension there must of necessity occur a change in the shape of the electrified body; and therefore, that on submitting a metal rod to such tension it must necessarily contract. To test the truth of this inference, the following experiments† were instituted alternately. A brass tube of about three feet long, and like-

* See *Jahrbücher des k. k. polytech. Institutes zu Wien*, vol. 17.

† They were performed at the Polytech. Institute at Vienna, about five years ago, with the aid of the Comparator, an instrument admirably calculated for such delicate measurements.

wise a solid bar of similar length, but not near so thick as the former, was laid upon insulating supports between the two feelers of a very sensitive arrangement of levers of contact, being however kept out of contact with them, by the insertion of strips of glass of suitable thickness.

Now immediately on receiving even a moderate charge of electricity, the index of the lever of contact began to move perceptibly, and to indicate that a gradual contraction of the bar was taking place, and this motion augmented so rapidly as the tension increased that, in order to enable the eye to follow the range of the index with greater facility, it became necessary to substitute a simple lever of contact, for the compound one which was at first employed. Every time the electric spark was drawn from the bar, or every time that it spontaneously discharged itself, the instantaneous recoil of the index of the lever, indicated the restoration of the original length of the metal; from which, however, there was again a transition to contraction, immediately the state of tension was renewed.

These experiments were repeated several times, and always with the same results, with however this difference, namely, that the contraction when the tube was employed was, probably on account of its greater extent of surface, much more marked than when the bar was used. These results are the more surprising, inasmuch as on account of the gradual increase of temperature, (for in these preliminary experiments, a single pair of galvanic elements was also used,) we should rather have looked for an expansion of the metal.

Now, though at the time of performing these experiments, I had reason to rest satisfied with having completely established what I had in view; yet, I now feel convinced from having subsequently thought the subject over, that the results then obtained, bear out certain inferences not perhaps altogether unimportant respecting the constitution and actual nature of the electric fluid. Nothing but the idea however, that this problematic phenomenon may be looked on by other experimenters as of sufficient importance, to have its existence completely established or disproved, by a repetition of my experiment, could induce me to lay it thus before the public, in a state so imperfect in many respects. And though for the present, that is to say, till it is established as an indisputable fact; I very properly refrain from expressing an opinion on the subject; yet I trust I may be permitted to subjoin a remark or two, and to allude, in passing, to an application of which, this new property of electrical tension is perhaps susceptible.

Simply putting this property of electrical tension beyond a doubt by careful and accurate experiments, would certainly—as far as it went, be a step in science; but the subject would gain additional interest, if in the investigation regard, was at the same time, paid not only to the length, but also to the shape and other qualities of the conductor, semi-conductor, or non-conductor. For in point of fact, it is by no means improbable that a contraction which is considerable enough to be measured and expressed in numbers, will turn out to be proportional to the length of bars of similar form, but that its amount will vary with the different metals employed. And this result may be especially anticipated in the case of such metals as indicate opposite states of electricity, as for instance, copper and zinc. It would, in fine, be well to enquire whether the same identical bar charged to an equal amount of tension, as indicated by the electrometer, first positively and then negatively, would indicate precisely the same amount of contraction.

Now should this power of electrical tension to contract metal rods so considerably (a fact of which, as matters now stand, I cannot entertain a doubt,) be really borne out by further experiments; the idea of having recourse to it for the construction of an electrometer on a new principle, suggests itself readily enough. Without entering into a discussion as to the best arrangement for such an instrument, I may be permitted to observe that probably any thin strip of metal, one of whose sides is covered with an elastic non-conductor; as for instance, a coat of elastic varnish, would, on being coiled up into a conical spiral, probably answer the purpose very well. One of its ends would have to carry an index, as is the case with a metallic thermometer, or would be made to communicate its motion to a lever.

The amount of contraction thus placed at our disposal and which, all things considered, is by no means inconsiderable, justifies the presumption that such an arrangement would furnish us with a very sensitive electrometer.

It will not perhaps be thought too much if in concluding this short communication, I express the hope that other observers will consider this phenomenon worthy of further notice and examination.

JULIAN GUGGSWORTH.

Wormwood Scrubs, 18th April, 1839.

LIV.—On the *Vindicating Electricity of Compact Solid Insulating Strata*.*

The first phenomena that have been observed with regard to the *vindicating* electricity of compact insulating strata, were those, a notice of which was sent by the father Jesuits at Pekin, to the academy of St. Petersburg, in the year 1755, and which may be read in the 7th volume of the new commentaries of this academy. Signor Symmer in his third Memorial, which was read in the Royal Society of London, the 20th of December, 1759, says he charged two thin sheets of glass, joined together by their naked surfaces, and externally coated; when the charge was completed, he took the upper plate, by two of its angles, and when he raised it, he saw that the under plate stuck to it, and remained suspended to it; when he had discharged the plates, the adhesion ceased. He recharged the two plates, then having inverted them when thus united, he made the plate that communicated at first with the chain, communicate now with the ground, and that which communicated with the ground, communicate with the chain; when he found that after the electrization had, in this state of things, been continued a certain time, all adhesion ceased. Using afterwards two plates coated on both their contiguous surfaces, he found that no adhesion took place. Signor Symmer makes use of these two experiments in order to confute the theory advanced by certain philosophers, of two electric fluids, the one *affluent*, the other *effluent*; he pretends that each of the two distinct united glasses may be considered as the one of the surfaces of a single plate; that one of the glasses is impregnated with an electricity of one kind, and the other glass with an electricity of another kind; he moreover is of opinion that the adhesion of the two naked plates of glass is a demonstrating proof of the existence of two antagonist forces.

Signor Cigna, in the fourth chapter of his dissertation, carried still farther the experiment of the fathers of Pekin, and of Signor Symmer. He relates that two naked glasses, by rubbing the upper surface of them, remained united, both to each other, and to the gilt paper, or the sheet of lead, on which they were placed; that in this state they gave no sign of electricity; that if they were then separated from the paper, or the lead, they manifested on their two external surfaces the same kinds of electricity; that if the paper or lead was again joined to the glasses, the electric signs again ceased; that if the paper, or lead, was kept parted from the glasses by means of a silk ribbon, the paper or lead manifested

* Beccaria's artificial electricity.

an electricity contrary to that of the glasses; that if the glasses were likewise kept separated from each other, they also manifest contrary electricities.

I do not propose to repeat all the numerous experiments which I related in my book intitled, *Observationes atque experimenta quibus electricitas vindex latè constituitur et explicatur*. I am actually employed in promoting my enquiries on this subject, and if I meet with some success, I propose to publish what discoveries I shall be able to make. Mean while I shall only repeat in this place, the experiment which is made with the two plates, A B, *a b*, M N, *m n*, (Pl. IX. fig. 1.) jointly charged, and I shall express the successive effects of the *vindicating* electricity in this experiment with the figure 2.

And first, in order to perceive the unity which really takes place in all the phenomena of the *vindicating* electricity, however contrary to each other some of them may appear, it must be observed, I. That the law of the *vindicating* electricity of compact insulating strata, for instance, plates of crystal, is the same with the law of the *vindicating* electricity of rare insulating bodies, for instance, silk ribbons.* II. That the whole specific difference between them lies in the former being capable of a charge, which the latter are not. III. Thence it results that the alterations of electricities, which are readily affected with bodies of a rare texture, by disjoining and rejoining them, and not so with compact insulating strata; such alterations are confined to those surfaces of the latter which are kept joined together by the contrary electricities of the other two surfaces, which constantly endeavour to preserve *their contrariety to each other, and their equality with the electricity of the surfaces which are united together*.

For instance, I. Two ribbons contrarily electrified, when they unite together, reciprocally destroy their electricities, and thus remain adherent. After the same manner, if two plates A B *a b*, M N *m n*, are joined by their respective surfaces, *a b*, M N, contrarily electrified (I suppose the surface A B to be positively electrified, and the opposite *a b*, negatively; therefore M N is positively electrified, *m n*, negatively) these two contrary electricities will endeavour to destroy each other; the redundant fire in M N will endeavour to diffuse itself into *a b*, and fill up its deficiency; but this reciprocal suppression of electricities cannot be effected otherwise than by a joint annihilation of the excess in A B, and of the deficiency in *m n*; therefore, in consequence of the impenetrability of the plates, some external communication becomes

* Beccaria gives a chapter on rare insulating bodies, in the same work.—EDITOR.

necessary; and this will no sooner be procured, than the excess of $M N$ will diffuse itself into $a b$, when the electricity of the two surfaces $a b$, $M N$, will be annihilated after the same manner as the electricities of the two ribbons were before.

Again, the two ribbons, when they are separating, freely recover their electricity, which they had readily lost when they joined; and in the same manner, the two plates $M N m n$, $A B a b$, in the instant they are separating, endeavor to recover on their surfaces $a b$, $M N$, the electricity they have lost in consequence of their union together, and of the communication of their external surfaces. Yet it is to be observed that the surface $M N$, in its endeavor to recover its excess, is restrained by the difficulty which the insulated opposed surface $m n$, experiences in dismissing an adequate part of its own fire; and the surface $a b$ likewise, in its endeavour to recover its deficiency, is restrained by the difficulty which the opposite surface $A B$ experiences in recovering an adequate excess; whence it happens that the two disjoined plates,—I. Manifest electricities reciprocally contrary; II. Similar electricities take place over the two opposite surfaces of the same plate; III. And this electricity is of the same kind as that recovered by the disjoined surface.

The reason is, that in disjoining the two surfaces, $a b M N$, I. The the surface $M N$, by endeavouring to recover its former excess, endeavors at the same time to drive away a quantity of natural fire from the opposed surface $m n$. Now, as the latter remains insulated, it cannot transfuse any fire into the ground, neither can it accumulate any within its coating $c d$; it therefore must accumulate it on the open surface of this coating, against the contiguous air: so that there will result an excessive tension in the natural fire of the ambient air, and a redundant atmosphere around $m n$. II. Likewise, in the act of the same separation, the surface $a b$, in endeavouring to resume its former deficiency, draws, according to the Franklinian theory, certain quantity of redundant fire, to the opposite surface $A B$: now, as this surface remains insulated, it cannot derive this fire from the ground, neither can it draw it from the internal substance of its own coating; it must then draw it from the outer surface of this coating, that is, from the surface of the contiguous air (if before separating the plates, the coatings are taken off, the experiment will equally succeed). Therefore, a particular relaxation will arise in the natural fire of the air around the plate $A B$; there will result a deficient atmosphere.

This explanation how the atmospheres arise, which take

place over the surfaces opposite to those which are disjoining, likewise suffices to explain the singular circumstance of similar electricities arising over opposite surfaces of the same plates. If while the two plates $A B a b$, $M N m n$ are separating, two sharp points are kept presented to their external surfaces, the brush appears on the point directed to $A B$, and the star on the other which is directed to $m n$: the same force which, when the points are presenting, draws a brush to $A B$, and drives the fire that forms another brush from $m n$, this same cause I say, when these two surfaces remain insulated, draws to $A B$ the natural fire of the contiguous air, creating a deficient atmosphere over it, and throws excessive fire from $n m$, into the air contiguous to it, raising in it a redundant atmosphere.

That afterwards, over the external surfaces correspondent to $a b$, $M N$, when they are separating, atmospheres arise that are homologous to the electricity which these surfaces recover, is what appears natural, when we consider, *that the latter surfaces resume, by virtue of their separation, greater electricities than those which can possibly be raised on the opposite surfaces, which are insulated.* This principle being admitted, it follows that if the surface $M N$, cannot drive from the opposite surface $m n$, a quantity of fire sufficient to produce in it a deficiency equal to the excess recovered by the same $M N$, it follows, I say, that a portion of this excess must flow outward, against the contiguous air, and there produce a redundant atmosphere. Likewise, if the surface $A B$ cannot draw to itself a quantity of fire sufficient to produce in it an excess equal to the deficiency recovered by $a b$, it follows that this $A B$ must, from the air contiguous to it, draw a certain quantity of fire, and thus produce a deficient atmosphere over itself. That is to say, the excess redundant in, and flowing of, $N M$, against the air contiguous to it, *ipso facto* lessens the excess in this $M N$, and thus brings it to a state of less inequality with respect to the deficiency actuated in $m n$; and the fire which from the contiguous air flows into $A B$, *ipso facto* lessens the deficiency in it, and thus brings it to a state of less inequality with regard to the excess in $a b$.

These explanations of the *vindicating* electricities of two plates, may be demonstrated by the experiment in which, after jointly charging and discharging them, I continue for an hour and more to obtain sparks by touching them when separated, and again touching them when rejoined; and reciprocally, the above explanations throw a complete light on that same experiment, which I never could repeat without exciting the wonder of those who were unacquainted with electrical

operations, and attracting the attention of the Philosophers who came to see my experiments. I join the two plates $A B a b$, $M N m n$ together, by their naked surfaces in contact with each other; and then introduce into the coating $C D$, for instance, the electricity of the chain; the charge completed, I discharge them; this done, I separate them, and touch the coatings; I join them again, and then again touch them; and thus doing, I continue to excite a very long series of sparks: here follows the manner after which I operate.

I begin with exciting sparks from the coating alone of the upper plate; that is to say—I. I continually touch with one of my fingers the under coating $c d$. II. When I separate the plate $A B$, I take care not to touch its coating $C D$. III. Having separated this plate, I immediately touch it, and give a spark to it; that is to say, I give to $A B$ an excess adequate to the deficiency contracted by $a b$, at the instant of the separation. IV. I cease touching $A B$; I rejoin the two plates, and touch again $C D$, and draw sparks from it; by means of which I draw off the excess I communicated to $A B$ after the last separation, and which it does no longer require, when in a state of conjunction. V. Proceeding thus, with the usual caution, not to touch the coatings in the act of separating, or of rejoining the plates, I continue to give sparks after every separation, and take them back after rejoining the plates.

In general the spark which I draw after rejoining the plates, is more divided than that which I gave after separating them. In very favourable weather, after separating the plates, I often draw two or more successive sparks; but after rejoining them, the fire that leaps from my finger is completely united into one spark, and much more vivid.

In order to understand the reason of this difference, we must consider—I. That the fire which flies from $a b$, in consequence of the deficiency which now takes place in it, goes to $M N$ in order to form the excess which this $M N$ wants; therefore as an excess arises in $A B$, in consequence of my touching it at times, so a deficiency arises in $m n$, in consequence of its constant communication with my hand. II. When I rejoin the two plates, the excess I have introduced into $A B$ cannot be annihilated but so far as the excess in $M N$ runs to fill the deficiency in $a b$; and the excess in $M N$ does not depart, but when I give fire to $m n$, in order to fill its deficiency. III. In fact, if, while I rejoin the plates, I keep my fingers at a distance from $m n$ (or its coating $c d$) then I cannot draw from $A B$ the excess I introduced into it; because as I do not then fill the deficiency in $m n$, the excess

cannot be annihilated in $M N$, nor the deficiency in $a b$ supplied. IV. However, when I touch $m n$ (or $c d$) while I rejoin the plates, the excess of $A B$ is not for all that thrown out at once, because the surfaces $a b M N$, do not instantaneously touch each other in all their parts; hence a slowness and successiveness take place in all the respective annihilations of the excess in $M N$, of the deficiency in $a b$, and of the excess in $A B$. V. But when after separating the plates I present my finger to $C D$, or $(A B)$ the excess is at once thrown to it from my finger, owing to the violence which the whole $A B$ then wants an excess adequate to the deficiency then completely formed in $a b$.

Conformably to what has been said above, we must take care that every time that the plates are joined, they be pressed together for some few seconds of time, in order that the small charges which have been formed by the separation, may have time both to dissipate entirely, and to arise again with more strength, when the separation will be again effected.

1. If after touching the plates when rejoined, they are again disjoined without drawing a spark, and then rejoined no spark will be thrown from $A B$, because it has in such case, received no fire. II. If after touching the plates when separated, they are rejoined, then disjoined again, without previously drawing a spark, $A B$ then receives no spark, because it has given none at the time of its last joining with the other plate; *so true it is that insulating bodies contrarily electrified, are disposed, when they join together mutually to annihilate their reciprocal electricities, as well as to recover them again, when they are separated.*

I have hitherto, in the experiment of the two plates, only examined that kind of electricity which is common both to compact insulating bodies, and to those of a rarer texture: I mean that kind of electricity, *by virtue of which they recover, when separated, the electricity which they had lost by their being joined together, and which I call positive vindicating electricity.* Now, I shall in the same experiment, examine that kind of *vindicating* electricity which is proper to compact insulating bodies, *and by virtue of which, when they are separating from one another, they give up the electricity with which they had been impregnated; this I call negative vindicating electricity.*

Having therefore jointly charged the two plates $A B a b$, $M N m n$, I begin the operation of successively disjoining and rejoining them: in order to effect this more easily, I clip one of the angles of one of the plates; and then I observe, I. That the plates, when they are disjoining, manifest signs of a

negative vindicating electricity. II. They afterwards reach to the last limits of this electricity. III. Then successively follow, for a very long space of time, to give signs of a *positive vindicating* electricity. That is to say, I. At first, the surfaces $a b$, $M N$, when they are separating, lose a part of the electricity with which they are impregnated. II. Then they reach a certain term at which they do not, notwithstanding they are again separated, lose any more of the electricity which remains in them, nor recover any portion of that which they gave up when the *negative vindicating* electricity began to act, or even afterwards when the *positive vindicating* electricity began to take place.

In the meanwhile, the similarity of the atmospheres that take place over the two surfaces of the same plate, both when the *positive vindicating* electricity, and the negative one obtain, though it has been looked upon as fatal to the Franklinian theory, really proceeds from the following principle, which is the foundation of this theory, which is, *that the contrary electricities of plates, which by virtue of the separation of the latter, are become unequal on each opposite surface, severally endeavour to return to a state of equality; that is to say, that electricity on the one of the two surfaces, which the separation has caused to have grown less, endeavours to lessen the electricity on the other surface; and vice versa, that electricity which, in consequence of the separation, is become superior to its opposite one, tends to increase the latter.*

Therefore, when I at first begin to separate the two plates $A B a b$, $M N m n$, the excess of $M N$ and the deficiency of $a b$ endeavour mutually to lessen each other; but the other two surfaces $A b$, $m n$, being insulated, their respective excess and deficiency are not altered; that is to say, the excessive fire is, as it were, drawn from $M N$ into $a b$; the deficiency in $a b$, thus become less than the excess in $A B$, and endeavours to lessen it; it therefore drives a portion of this excess in $A B$, against the air contiguous to it, and thus creates the redundant atmosphere over $A B$: and reciprocally, the excess in $A B$ being now greater than the deficiency in $a b$, endeavours to increase it; it drives a part of the fire remaining in this $a b$, into the air contiguous to it, and raises over it a redundant atmosphere. Likewise, the excess in $M N$ being become less than the deficiency in $m n$, endeavours to lessen it, it draws fire into $m n$ from the air contiguous to it, and thus renders its atmosphere still more deficient; and reciprocally, the deficiency in $m n$, being greater than the excess in $M N$, endeavours to draw fire into the latter, from the air contiguous to it, and thus raises a deficient atmosphere over it.

On the other hand, when after the rise of the positive *vindicating* electricity, I again separate the plates, both the excess in *M N*, and the deficiency in *a b*, continue to be reproduced, though the contrary correspondent electricities cannot arise on the surfaces *A B, m n*, which remain insulated: therefore the greater deficiency in *a b*, endeavours to increase the lesser excess in *A B*, by drawing the natural fire from the contiguous air into it, and thus raises over *A B* a deficient atmosphere; and reciprocally, the less excess in *A B* endeavours to lessen the deficiency in *a b*; to that end it draws fire into it from the air contiguous to it, and thus raises over it a deficient atmosphere. Likewise, the greater excess in *M N* endeavours to increase the deficiency in *m n*, driving its fire from it into the air contiguous to it, whence results a redundant atmosphere over *m n*; and reciprocally, the less deficiency in *m n* endeavours to lessen the excess in *M N*; to that end it drives a part of the latter's redundant fire into the air contiguous to it, and thus raises a redundant atmosphere over it.

Conformably to these principles. I. When I separate the plates *A B a b, M N m n*, for the first time after their being charged, they resist so much the separation, that there is great danger in breaking them. II. From the coating *C D* a strong spark leaps to the nearest finger of that of my hands which holds the plate *A B a b*, and the edge of its coating *C D* appears all round sparkling with very vivid brushes: all this demonstrates to me that a diminution of the excess of *A B* takes place, at the instant when the deficiency of *a b* is forcibly lessened. III. Likewise, in the act of the same separation, a strong spark flies from the finger with which I hold the plate *M N m n*, to its coating *c d*, and its edge appears all round shining with vivid sparks; this manifests to me that a diminution of the deficiency of *m n*, is effected at the same time that the excess of *M N* is forcibly lessened. IV. Meanwhile, the flashes of light which appear between the surfaces *a b, M N*, while they are separating, are produced by the fire which, by virtue both of the excess in *A B* which remains superior to the deficiency in *a b*, and of the deficiency in *m n*, which remains superior to the excess in *M N*, endeavours to leap from the above *a b* into *M N*. V. In this state of things, the upper plate *A B a b* repels the white ribbon from both its surfaces; over which, as has been explained in the preceding paragraph, similar redundant electricities take place. VI. On the contrary, the under plate, *M N m n*, repels a black ribbon from both its surfaces, by virtue of the deficient

atmosphere, which as hath been also explained, takes place over both its surfaces.

The plates being joined again, the intensity of these attractions and repulsions lessen; because the excess of $M N$, and the deficiency of $a b$ are now respectively kept back by the external deficiency of $m n$, and the external redundancy of $A B$. The adhesion of the plates takes place again, but in a less degree than formerly, proportionably to the diminution which the original charge has suffered from the first separation; and by proceeding to a second separation, the same phenomena continue to take place by virtue of the same causes as formerly, though their intensity is proportionably lessened.

Continuing thus to join and separate the plates, we pretty soon attain a term at which, I. The plates cease to manifest any sensible adhesion. II. In separating them no light appears. III. After the separation, they do not sensibly draw or attract rubbed ribbons. This term is the point of the contrary inflexion, the limit between the negative *vindicating* electricity which takes place at first, and the positive one which succeeds to it. This term is sooner attained, according as the insulation of the plates is less complete: in this case one plate sometimes reaches to this term a little before the other, which still continues to draw and repel ribbons with a sensible degree of force. Lastly, this term is attained, before the effect of the separations has entirely annihilated the charge introduced at first into the plates. In fact, if they are rejoined immediately after the term is passed, they still give pretty strong shocks.

If, after the term is passed, the plates are successively joined and separated, but without touching them; they begin, by virtue of these successive separations, to recover their former electricities: that is, the surface $a b$ of the plate $A B$ begins to recover a part of what deficiency it had at first, and the surface $M N$, begins to recover also a part of what excess it may have lost. Whence it happens that, after the separation, the deficiency of $a b$, being become greater, endeavours to increase the excess of $A B$, by drawing into it the natural fire of the air contiguous to it; and reciprocally, the excess of $A B$, being less than the deficiency $a b$ endeavours to lessen it, by drawing into the same $a b$, the natural fire of the air contiguous to it; so that $a b$ and $A B$ then begin to repel the black ribbon. Likewise the excess of $M N$, being become greater than the deficiency in $m n$, endeavours to increase it, by driving the fire of $m n$ into the air contiguous to it; and reciprocally, the deficiency of $m n$, being less than the excess of $M N$, endeavours to diminish it, by driving

the fire of $M N$ into the contiguous air, whence $M N$ and $m n$ begin to repel the white ribbon. And thus the *negative vindicating* electricity becomes changed into a *positive vindicating* electricity.

By continuing thus to rejoin and disjoin the plates, those portions of electricity that had been lost are pretty quickly recovered on all sides, by virtue of these successive separations; the adhesion of the plates, and the repulsion of the ribbons also increase in proportion; so that it appears that all these phenomena of the *positive vindicating* electricity, continue till that degree is attained, at which the charges that had been introduced are annihilated.

Beyond this term, if the plates are continued to be re-joined and disjoined, for an whole hour or more, without being touched, they continue to shew some adhesion to each other; they continue when separated, to repel ribbons conformably to the kind of electricity which they have resumed on their internal surfaces, &c.

I have represented in the fig. 2. of the Pl. IX. the series of the above alterations of the vindicating electricity. Now I shall make use of this figure, in order to explain the vindicating electricity of the plate $M N m n$, (Pl. IX. fig. 1.) The same explanation will serve for the electricity of its fellow-plate; only, the ordinates must be taken on the other side of the absciss. Let the two equal right lines $O F$, $o F$ represent the excess introduced into $M N$ by the charge, and the deficiency introduced into $m n$. On the first separation of the plates, $M N$ will, for instance, lose the portion $u F$ of its excess: therefore, it will in consequence of this separation appear negatively electrified over both its surfaces; the plates being joined again, it will recover part of its former excess, and will thus be brought to have then the whole of its excess equal to $P G$. In consequence of a new separation, a portion $x G$ of the same excess will again be lost; and thus it will at last happen, that $M N$ will have that precise degree of excess at which a further separation can no longer lessen it; so that H is the point at which the *vindicating* electricity begins to be altered, that is, from negative becomes positive. At a following separation, by virtue of which the remaining excess is already reduced to the less value $R I$, the plate, instead of continuing to lose any more of its excess, on the contrary begins to recover the portion of it $I y$. Hence, as the remaining excess from the charge, in $M N$, is gradually reduced to to the less values $K S$ in K , $L A$ in L , and o in M , the surface $M N$ gradually recovers greater portions of its former excess, $K s$, $L z$, $M g$. From that point afterwards

the surface $M N$, by virtue of other successive separations, will for a very long while continue to recover portions of its former excess, which (the operation being continued without touching the plates) will gradually vanish at every successive conjunction of the same.

And thus the portions of a curve $O Q M$, $o q M$, will, with their respective ordinate, express the excesses and deficiencies, both primitive and remaining, of $M N$ and $m n$; the portions of a curve $\alpha H \& v$, $V H \& \bar{V}$, will, with their ordinates, express as far as H , the negative *vindicating* electricities, and beyond H , the *positive vindicating* electricities, of the surfaces $M N$, $m n$. The same portions of the curve which serve to express the degrees of positive and negative vindicating electricities that take place at every successive separation of the plates, will also serve to represent the progression of the mutual adhesion of the plates. αF , $U F$ will express the greatest degree of the adhesion of the plates, when they still retain their whole charge; which value will gradually lessen conformably to the successive lessening ordinates, $x G$, $X G$; at the instant when the negative electricity will take place, this value will be o in H , that is, at the point of the contrary inflexion; and thence it will continue quickly increasing, then very slowly decreasing, conformably to the successive ordinates, $I y$, $I Y$, $K s$, $K S$, $L z$, $L Z$, $M \&$, $M \bar{\&}$, &c.

With respect to the experiments that are made on the *vindicating* electricity of a single plate $A B a b$ (Pl. IX. fig. 3.) by disjoining its coating $C D$, they differ much in point of intensity and duration, from the experiments that are made with the two plates jointly charged. Of this difference the cause partly at least is manifest: in the separation of the two plates jointly charged, the *vindicating* electricities of the two surfaces which are disjoining, co-operate together; and this circumstance must increase the effects, and better preserve the efficient causes; that is, the dispositions introduced by the charge of the plates, by virtue of which they endeavour to dismiss their respective electricities to a certain degree, and beyond this degree, to recover the same.

With regard to the manner after which the same vindicating electricities exert themselves, I observe, I. That positive *vindicating* electricities exert themselves after the same manner, when only one plate is used, and separated from its coating, as when both are used, and successively separated from each other. II. Negative vindicating electricities also exert themselves after the same manner, if the charge introduced into the single plate is very weak, consisting for instance, of only two or three sparks from the first conductor; because the

charge which is usually introduced into the joined plates, is likewise small, on account of the thickness of the whole. III. But if the charge introduced in the single plate be much intense, then the phenomena which result from disjoining the coating of it, while the plate retains its whole charge, are proportionably different from the phenomena which result from separating the two plates, when they only possess their *joint* charge.

That is to say, each of the plates that retain their charge, manifests in consequence of a separation, the same electricity on both its surfaces, with that of the surface which is disjoined; but the plate which has been charged alone, and possesses a considerable degree of charge, manifests that kind of electricity on the surface which is disjoined from its coatings, which is proper to that surface; and the contrary kind of electricity on the other surface. Thus, if the single plate *A B a b* be strongly charged, positively in *A B*, and negatively in *a b*, it will, after the coating *C D* is taken off, repel a white ribbon from *A B*, and a black ribbon from *a b*.

The reason of this is, that charges universally endeavour, with a force proportioned to their intensity, to grow gradually less; and this force counteracts the force with which they endeavour to keep their state of mutual equality, the force by which the single charged plate endeavours, when separated from its coating, to *actuate similar atmospheres in the air contiguous to its two surfaces*. When I take off the coating *C D* from *A B a b*, which I suppose to be strongly charged, I lessen the electricity of *A B*; therefore, by virtue of the force with which the two contrary electricities constantly endeavour to keep their state of equality, the deficiency in *a b* must lessen, and the excess in *A B* of course somewhat increase: as the electricity on both surfaces strongly endeavours at the same time to grow less in consequence of its very intensity, the deficiency in *a b* very strongly lessens by the united efficiency of the two above causes, and the excess of *A B*, even after the separation of its coating, will continue to decrease a little, in consequence of the lessening force, which arises from the intensity of its charge, and surpasses that which tends to an equality; thence, a certain quantity of fire flows from *A B* into the contiguous air; but *a b* at the same time draws fire from the air contiguous to it with very great force, and after this manner the above effects take place.

I have repeated the above observations from my above mentioned book on the *vindicating electricity*, and added some new ones, in order to throw some more light on the subject; with regard to the nature of the adhesion which accompanies

vindicating electricities, I shall only subjoin two trials I have made about it. The first is as follows; if two plates, either charged, or lately discharged, and which therefore strongly adhere to each other, are immersed into an extensive subtle flame, or, when taken from this flame, are suspended within a large glass bell, emptied of air they soon part from each other. The other experiment is that of disjoining bodies naturally joined, for instance, strata or sheets of talc, or of *spato*: no electricity at all arises from these bare separations. With respect to the cause of the *vindicating* electricity, and of the adhesion that accompanies it, it certainly would, if discovered, throw a considerable light on the properties of insulating bodies, on the manner of their charges on the nature of electric atmospheres, and consequently on all the most striking phenomena of electricity, such as the *brush* the *star*, and the electrical motions. A consideration this which is very apt both to excite us to investigate such cause, and restrain us from barely *imagining* it.

L V.—*Synoptic View of the precise amount of pure Carbon, yielded by the rigid analysis from the Charcoals of thirty principal known Woods; by W. F. WEEKES, Esq., Surgeon. Lecturer on Philosophical and Operative Chemistry, &c, &c., Sandwich.**

Some twelve years since I was induced from circumstances arising out of engagements in the laboratory, to undertake a somewhat extensive series of experimental researches relative to gaseous, liquid and other products of numerous specimens of ligneous fibre, exotic as well as indigenous; subsequent to which course of enquiry, the *charcoals* of the respective woods were made the subject of extremely cautious analysis. From my minutes of the results then obtained, I select thirty of the principal specimens, and have brought them into a tabular view, under the impression that it is a point of some importance to the chemist and man of general science, as well as to certain manufacturers and others, to possess a source of reference upon which may be placed unqualified reliance, as respects the per centage of *pure carbon*, generally present in the charcoals from various specimens of wood; though I am aware that some few results of this description have already been given to the scientific world, by analytical chemists of no small celebrity. I shall only further observe, that the whole series of charcoals was obtained by close distillation from woods cut down in their full vigour, and afterwards

* Communicated by the Author.

gradually dried by exposure to the atmosphere. The following synopsis is arranged in the order of their purity downwards :—

CHARCOALS.	Amount of Pure Carbon in 100 grains.
Mulberry	99,50
Chestnut.....	99,38
Yew	99,05
Birch	99.
Cherry	99.
Box.....	98,75
Maple	98,75
Sycamore	98,75
Ash.....	98,75
Cedar	98,75
Lime	98,75
Holly	98,40
Lignumvite	98,40
Willow	98,25
Beech	98,13
Pear	98,13
American Oak	97,50
Hawthorn	97,50
Laburnum	97,50
Poplar	97,50
Alder	97,25
Evergreen Oak	96,88
Plum	96,87
Mahogany	96,25
Elm	96,25
Apple	96,25
English Oak	95.
Walnut	93,75
Ebony.....	92,40
Lancewood.....	86,25

Hence it will appear that between the two extremes of the table, mulberry and lancewood, independent of variations in the intermediate series, there exists a difference in purity amounting to 13,25 grains per cent.; and it may be further worthy of remark that, notwithstanding the striking want of uniformity in the *external* character of many woods, precisely the same amount of pure carbon appears to be essential to their constitution.

LVI.—On Tornadoes and *Ørsted's Memoirs respecting them*. By ROBERT HARE, M. D. Professor of Chemistry, in the Pensylvanian University, Philadelphia.

TO THE EDITORS OF THE NATIONAL GAZETTE.

Dear Sirs,—I believe it is generally admitted by electricians that the enormous discharges of the electric fluid, which, during thunder gusts, take place in the form of lightning, are the consequence of the opposite electrical states of an immense stratum of the atmosphere coated by the thunder clouds, and a corresponding portion of the terrestrial surface. In a memoir published in the 5th volume of the American Philosophical Transactions, republished in Silliman's Journal, volume 32, for 1837, I had endeavoured to show that the tornado was the consequence of the same causes producing, in lieu of lightning, an electrical discharge by a vertical blast of air, and the upward motion of electrified bodies. In your Gazette of the 30th ult., you have re-published an article by the celebrated *Ørsted* in which it is alleged that tornadoes or waterspouts cannot be caused by electricity, because there is no evidence proving that persons exposed have experienced electrical shocks. To me it appears evident that the scientific author confounds the different processes of discharge to which I have alluded, the one occurring in thunder gusts, the other in tornadoes; also that he has forgotten that a shock can be given neither by a blast of electrified air, nor by a continuous electrical current, a transient interruption of the circuit being indispensable to the production of the slightest sensation of that nature. If a person, having a conducting communication between one of his hands and a charged surface of a well insulated battery, hold in the other hand a pointed wire, the battery will be discharged through him and through the wire, producing a blast of electrified air from the point, without his experiencing any shock; neither would a shock be given to any person by exposure to the blast thus produced.

This form of electrical discharge to which I ascribe tornadoes, in which electricity is conveyed from one surface to another by the motion of air or other moveable bodies intervening, is by Faraday designated as "*convection*," from the Latin "*conveho*," to carry along with.

In the comparatively minute experiments of electricians, the process of convective discharge, is exemplified not only by the electrified aerial blast, but likewise by the play of pith balls, the dance of puppets, or the vibration of a pendulum, or bell clapper. The passage of sparks is found to arrest or to check such movements, and in like manner the passage of lightning

has been observed to mitigate the vertical force of a tornado.

While a meteor of this kind, which passed over Providence last year, was crossing the river, the water, within an area of about three hundred feet in diameter, was found to rise up in a foam, as if boiling. Meanwhile two successive flashes of lightning occurring, the foam was observed to subside after each flash. It is thus proved that a discharge by lightning is inconsistent with the discharge by convection, and that so far as one ensues, the other is impeded.

In an account of a tremendous storm of the kind of which I have been treating, published in Silliman's Journal for July last, it is mentioned, that, at its commencement, it was only a violent thunder gust. This is quite consistent with the experience acquired by means of our miniature experiments, in which a discharge, by sparks, may be succeeded by a discharge by convection, or vice versa, or they may prevail alternately. In one case the electric fluid passes in the gigantic sparks called lightning, in the other it is conveyed by a blast of electrified air. In the former case animals are subjected to deleterious shocks, while in the latter no other injury is sustained than such as results from collision with the air, or other ponderable bodies.

In the case of the tornado, the vertical blast is accelerated by the difference between the pressure of the air at the earth's surface, and at the altitude to which the blast extends. Should this be a mile there would be a difference nearly of one hundred and forty-four pounds per square foot. During the tremendous gale which prevailed at Liverpool last winter, the greatest pressure of the wind was estimated at only thirty pounds per square foot. So far as the ingenious inferences and observations of Mr. Epsy, as to the buoyancy resulting from a transfer of heat from aqueous vapour to air hold good, the vertical force so alleged to arise, will co-operate to aid the influence of electric discharges by convection.

The distinguished author of the memoir alluded to at the outset of this communication, conceives that were electricity the cause of tornadoes, the magnetic needle should be disturbed by them; and without advancing any proof that such disturbance does not take place, founds thus an objection to electrical agency. I conceive that it would be unreasonable to expect a magnetic needle to be affected by an electrified blast of air, if protected from its mechanical force.

It has been shewn, by Faraday, that without peculiar management, tending to prolong the re-action, the most delicately suspended needle cannot be made to diverge in obedience to the most powerful discharges of mechanical electricity. An electrical spark may impart a feeble magnetism,

but it is too rapid and transient to effect a needle. Moreover, when a needle is at right angles to an electric current, which would be quite competent to influence it, if parallel to it, there can be no consequent movement, since the current tends to keep it in that relative position. The direction of every electrical discharge, inducing a tornado, must necessarily be nearly at right angles to the needle, since it must be vertical, while the needle is necessarily horizontal, when so supported as to traverse with facility.

I do not perceive any facts or suggestions in the article by CErsted, which are competent to render the phenomenon of which he treats more intelligible than it was rendered by the accurate survey and examination of the track of the New Brunswick tornado, by Dallas, Bache, and Espy, in connexion with accounts published by other witnesses of that and other similar meteors.

It seems to be admitted, on all sides, that within a certain space there is a rarefaction of air, tending to burst or unroof houses. That the upward blast consequent to this rarefaction, carries up all moveable bodies to a greater or less elevation; that an afflux of air ensues, from all quarters, to supply the vacuity, which the vertical current has a tendency to produce. Trees, within the rarified area, are uprooted, and sometimes carried aloft; but on either side of it, or in front, or in the rear, are prostrated in a direction almost always bearing towards a point, which during some part of the time in which the meteor has endured, has been under the axis of the column which it formed.

It appears to me that all the well authenticated characteristics enumerated by CErsted, are referable to the view of the case thus presented. This distinguished author assumes that there is a *whirling* motion, although between American observers this is a debated question. It seems in the highest degree probable that gyration does take place occasionally, if not usually, since in the case of liquids rushing into a vacuity, a whirlpool is very apt to ensue. But as slight causes will in such cases either induce or arrest the circular motion, such movements may be contingent. It would however appear probable that when gyration does exist, it may, as the consequent generation of centrifugal force tend to promote or sustain the rarefaction, and thus contribute to augment the force, or prolong the duration of a tornado.

From observations made upon the track of the recent tornado at New Haven, I am led to surmise that there was more than one axis of gyration and vertical force—I conceive that in consequence of the diversities in the nature of the

bodies or the soil, there was a more copious emission of electricity from some parts of the rarefied area than others. In two instances waggons with iron wheel tires and axles, were especially the objects of the rage of the elements. Trees equally exposed were unequally affected, some being carried aloft, while others were left standing. The area of a tornado track may be more analogous to a rough surface than a point, and the electricity may, from its well known habitudes, be given off from such bodies as are from their shape or nature most favorable to its evolution.

Since these inferences were made, I have observed in Reid's work upon Storms, that similar impressions were created by facts observed during a hurricane at Mauritius in 1824. It was remarked that narrow, tall, and decayed buildings, ready to turn into ruins escaped, at but little distance from new houses which were overturned or torn into pieces. It was inferred there were local whirlwinds, subjecting some localities to greater violence than others in the vicinity. In the case of other hurricanes similar facts have been noticed.

It may be expedient here to subjoin, that I consider a hurricane as essentially a tornado, in which an electric discharge by "*convection*," associated with discharges in the form of lightning, takes place from a comparatively much larger surface. In the case of the hurricane, however, the area of the track is so much more extensive, that the height of the vertical column to the diameter of the base being proportionably less, there is necessarily a modification of the phenomena, which prevents the resemblance from being perceived. In the case of the hurricane, the column is too broad to come within the scope of a human eye.

So much has lately been presented to the public, either through the newspapers, journals, or lectures, which I consider demonstrably incorrect that I can hardly, consistently with my love of true science, remain an inactive observer of the consequent perversion of the public mind. Unfortunately it is difficult, if not impossible to discuss such subjects without a resort to language and ideas, which are too technical and abstruse for persons who have not made chemistry and electricity an object of study.—I have however prepared a series of essays, in which the causes of storms are stated, agreeably to my view of this important branch of meteorology.—I am, gentlemen, yours truly,

ROBERT HARE.

LVII.—*An account of a remarkable Tornado which occurred towards the last of June, at Chatenay, near Paris, being translated from the Report of a Parisian savant, Peltier, appointed to ascertain whether insurers were liable for the losses under policies against damage from thunder storms (See Journal des Debats for the 17th of July.) Also Remarks and Annotations by R. HARE, M. D. Professor of Chemistry, in the Pensylvanian University, Philadelphia.*

FOR THE NATIONAL GAZETTE.

Messrs. Editors :—You had published a memoir on Tornadoes by a distinguished foreigner, CErsted. Conceiving the impression conveyed by that article less worthy of consideration than those which had been presented in a memoir which I had previously published, I hope that I shall be considered as having had a sufficient incentive for endeavouring through the same channel to correct the erroneous impressions which that memoir was in my opinion of nature to produce.

In my letter to you of the 26th ult. it was stated that I considered tornadoes as the consequence of an electrical discharge superseding the more ordinary medium of lightning. From an acticle which has since met my attention in the Journal des Debats, published on the 17th July at Paris, it appears that a tremendous tornado occurred about the last of the preceding June in the vicinity of that metropolis. The losers applied for indemnity to certain insurers, who objected to pay on the plea that the policies were against thunder storms, not against tornadoes. This led to an application to the celebrated Arago, who referred the case to another savant, Peltier.

From the report of Peltier, of which I subjoin a translation, it will be seen that, excepting his neglect of co-operative influence of the elasticity of the air, he sanctions my opinion that a tornado is the effect of an electrical discharge.*

* I had presented copies of the pamphlet containing my memoir to M. Arago and several other members of the institute. In a subsequent conversation he referred to some of the suggestions which it contained. As it conveyed a view of the question decisively favourable to the claimants, it may be inferred that it must have been alluded to by Arago and thus have become the source of Peltier's impressions. It may therefore be anticipated that due acknowledgment will be hereafter made by him when he realises his promise of making a more elaborate report on the tornado of Chatenay. Before entering upon the arguments by which I sustained my hypothesis it was briefly stated in the following words: "*After maturely considering all the facts I am led to suggest that a tornado is the effect of an electrified current of air superseding the more usual means of discharge between the earth and clouds, in those vivid sparks which we call lightning.*"

"Yesterday," says Peltier, "I visited the commune of Chatenay in the canton of Ecouen, department of Seine and Oise, and investigated the disasters experienced in the month of June last, from a tornado which first originated over the valley of Fontenay des Louvres. At present I can give only a summary account of this wonderful phenomenon.

"Early in the morning a thunder cloud arose to the south of Chatenay, and moved at about ten o'clock over the valley between the hills of Chatenay and those of Ecouen. The cloud having extended itself over the valley, appeared stationary and about to pass away to the west. Some thunder was heard but nothing remarkable was noticed, when about mid-day a second thunder storm coming also from the south and moving with rapidity advanced towards the same plain of Chatenay. Having arrived at the extremity of the plain above Fontenay, opposite to the first mentioned thunder cloud, which occupied a higher part of the atmosphere, it stopped at a little distance, leaving spectators for some moments uncertain as to the direction which it would ultimately take. That two thunder clouds should thus keep each other at a distance, led to the impression that being charged with the same electricity, they were rendered reciprocally repellent, and that a conflict would ensue in which the terrestrial surface would play an important part. Up to this time there had been thunder continually rumbling within the second thunder cloud, when suddenly an under portion of this cloud descending and entering into communication with the earth, the thunder ceased. A prodigious attractive power was exerted forthwith, all the dust and other light bodies which covered the surface of the earth mounted towards the apex of the cone formed by the cloud. A rumbling thunder was continually heard. Small clouds wheeled about the inverted cone rising and descending with rapidity. An intelligent spectator, M. Dutour, who was admirably placed for observing, saw the column formed by the tornado terminated at its lower extremity by a cap of fire; while this was not seen by a shepherd, Oliver, who was on the very spot, but enveloped in a cloud of dust.

"To the south-east of the tornado, on the side exposed to it, the trees were shattered, while those on the other side of it preserved their sap and verdure. The portion attacked appeared to have experienced a radical change, while the rest were not affected. The tornado having descended into the valley at the extremity of Fontenay, approached some trees situated along the bed of a rivulet, which was without water though moist. After having there broken and uprooted every tree which it encountered, it crossed the valley and advanced

towards some other trees, which it also destroyed. In the next place, hesitating a few moments as if uncertain as to its route, it halted immediately under the first thunder cloud. This, although previously stationary, now began as if repelled by the tornado to retreat towards the valley to the west of Chatenay. The tornado after stopping as I have described, would infallibly on its part, have moved on towards the west to a wood in that direction, if the other thunder cloud had not prevented it by its repulsion. Finally it advanced to the park of the castle of Chatenay, overthrowing every thing in its path. On entering this park, which is at the summit of hill, it desolated one of the most agreeable residences in the neighbourhood of Paris. All the finest trees were uprooted, the youngest only, which were without the tornado, having escaped. The walls were thrown down, the roofs and chimneys of the castle and farm house carried away, and branches, tiles and other moveable bodies were thrown to a distance of more than five hundred yards. Descending the hill towards the north, the tornado stopped over a pond killed the fish, overthrew the trees, withering their leaves, and proceeded slowly along an avenue of willows, the roots of which entered the water, and being during this part of its progress much diminished in size and force, it proceeded slowly over a plain, and finally at the distance of more than a thousand yards from Chatenay, divided into two parts, one of which disappeared in the clouds, the other in the ground.

“ In this hasty account I have, with the intention of returning to this portion of the subject, omitted to speak particularly of its effects upon the trees. All those which came within the influence of the tornado, presented the same aspect; their sap was vaporized, and their ligneous fibres had become as dry as if kept for forty-eight hours in a furnace heated to ninety degrees above the boiling point. Evidently there was a great mass of vapour instantaneously formed, which could only make its escape by bursting the tree in every direction; and as wood has less cohesion in a horizontal longitudinal, than in a transverse direction, these trees were all, throughout one portion of their trunk, cloven into laths. Many trees attest, by their condition, that they served as conductors to continual discharges of electricity, and that the high temperature produced by this passage of the electric fluid, instantly vaporized all the moisture which they contained, and that this instantaneous vaporization burst all the trees open in the direction of their length, until the wood, dried up and split, had become unable to resist the force of the wind which accompanied the tornado. In contemplating the rise and progress of this phe-

nomenon, we see the conversion of an ordinary thundergust into a tornado;* we behold two masses of clouds opposed to each other, of which the upper one, in consequence of the repulsion of the similar electricities with which both are charged, repelling the lower towards the ground, the clouds of the latter descending and communicating with the earth by clouds of dust and by the trees. This communication once formed, the thunder immediately ceases, and the discharges of electricity take place by means of the clouds which have thus descended and the trees. These trees traversed by the electricity, have their temperature, in consequence, raised to such a point that their sap is vaporized, and their fibers sundered by its effort to escape. Flashes and fiery balls and sparks accompanying the tornado, a smell of sulphur remains for several days in the houses, in which the curtains are found discoloured. Every thing proves that the tornado is nothing else than a conductor formed from the clouds, which serves for a passage for a continual discharge of electricity from those above, and that the difference between an ordinary thunder-storm and one accompanied by a tornado, consists in the presence of a conductor of clouds, which seems to maintain the combat between the upper portion of the tornado and the ground beneath. At Chatenay this conductor was formed by the influence of an upper thunder cloud, which forced the lower portion of an inferior cloud to descend and come into contact with the terrestrial surface."

Peltier concurs with me in the opinion that the tornado supersedes lightning, by affording a conducting communication between the terrestrial surface and thunder cloud: but he conceives that the cloud, by its descent, becomes the conductor, through which the electric discharge is accomplished: whereas, agreeably to the explanation which I suggested, a vertical blast of air, and every body carried aloft, contributes to form the means of communication. Agreeably to this suggestion, the electric fluid does not pass by conduction, but "convection," as explained in my letter of the 26th ult. That the idea of the parisian savan, that the cloud acts as a conductor, is untenable must be evident, since the light matter of which a cloud is constituted could not be stationary, between the earth and sky, in opposition to that upward aerial current of which the violence is proved to be sufficient to elevate not only water, but other bodies specifically much heavier than this liquid.

* See 5th vol. of the American Philosophical transactions, or Silliman's Journal for 1837, vol. 32, page 154.

So much of the narrative of Peltier as relates to the repulsion between the thunder clouds, is inconsistent with any other facts on record respecting tornadoes which have come within my knowledge. It should be recollected that this part of the story does not depend upon the observation of the author, and may be due to the imagination of the witnesses whom he examined. The most important part of his evidence, is that respecting the effect upon the trees, which appears to me to demonstrate that they were the medium of a tremendous electrical current.

In my memoir I noticed the injury done to the leaves of trees, and stated my conviction that "*as it was inconceivable that mechanical laceration could have thus extended itself equally among the foliage, a surmise may be warranted that the change was effected by electricity associated with the tornado.*"

LVIII.—*Description of a new Voltameter.* By MARTYN ROBERTS, Esq. In a letter to the Editor.

MY DEAR SIR.—If you think the following account of an instrument worthy of a place in your *Annals of Electricity*, you are at liberty to insert it. I contrived the instrument last winter, and found it exceedingly useful in comparing the decomposing power of different electric currents. I brought it before the Royal Society of Edinburgh, where it was much approved of.

The usual way of measuring the quantity of gas developed by the poles of a galvanic battery, is by an instrument called a voltameter, of which there are many forms; but to all there is an objection, viz., the trouble, and often difficulty of refilling the tube with the liquid to be decomposed. The change I have made in the form makes it a very simple instrument, giving great facility of manipulation, which you will allow is of importance in all electrical experiments. My voltameter fig. 5. pl. IX. is a glass tube, bent like the letter U, and sunk into a wooden stand, as deep as the dotted lines in the figure. One leg *a* will contain about three cubic inches of gas, and on its length, is cut a scale dividing it into inches and tenths, cubic; on the summit of the other leg *b* is a reservoir *c* which will contain something more than three cubic inches.

About an inch above the lowest point of the curvature of the tube, and in the leg *a* two holes are bored in the glass, and in these are cemented two short pieces of No. 6 platina wire. *d. d.* The ends of these wires in the tube, must be close to each other, but must not touch. The outward ends of these

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wires terminate in two binding screws, *s, s*, for the purpose of attaching to them the wires of a battery. On the summit of the leg *a* is a stop-cock.

To use the instrument, fill both legs with dilute sulphuric acid to the level of the stop-cock, or rather to zero on the scale. Shut the stop-cock, and fasten the battery wires in the binding screws: the decomposition of the water now commences, the gas rises in the leg *a* and the liquid is raised into the reservoir *c* and this will continue until the liquid is depressed in the leg *a* below the platina wires. The number of inches and tenths, of gas produced in a given time is marked by the scale, and gives, of course, the comparative power of the battery as usual. But now if you wish to repeat the experiment, you have only to open the stop-cock, the gas rushes out, and the apparatus is instantly ready for another trial.

I remain, my dear Sir, yours truly,

MARTYN J. ROBERTS.

LIX.—*On an Air Electrometer*; by B. W. COWARD, Esq.
In a Letter to the Editor. See fig. 4. Pl. IX.

DEAR SIR,—The instrument consists of a glass cylinder, three inches diameter, by eight inches in length, on each end of which a brass cap is cemented air tight; passing through the upper cap, and near the edge is a glass tube B blown with a funnel-shaped end (for the purpose of exposing a greater surface,) and bent so as to leave a short parallel arm of about two inches and a half. To the long arm of this tube, a narrow graduated scale of ivory is affixed by means of fine wire. C and D are brass wires and balls placed in the centre of the caps, the upper one sliding in a collar of leather. In order to use this instrument, the tube B must be filled to about the height of two inches, with a fluid, on the surface of which in the long arm must rest a light guage made of ivory, and sliding so freely as to require very slight springs made of quill, to restrain it by thin pressure in any part of the tube.

Now it is evident if a charge be passed through the cylinder, the air in it will be displaced, and pressing down the fluid in the short arm, it will rise in the long one, and of course the guage with it, which by the springs, will be restrained at its maximum height. The guage is represented at E.

The advantages to be derived from this construction of the instrument, I conceive to be,—

- 1st. The appearance is more elegant.
- 2nd. It is more easily affected.

3rd. There is no slopping about of a large quantity of fluid in the bottom of the cylinder.

4th. Should the tube require cleaning, or the fluid replenishing, it is easily effected.

5th. The permanent indication afforded by the gauge, of height to which the fluid has risen.

LX.—*The Aurora Borealis, of September 3d, 1839.*

A very singular aurora borealis appeared at London, on the evening of the 3rd of September, 1839. It first made its appearance about a quarter before nine o'clock, and continued nearly the whole of the night. I was walking from Brixton to Peckham, between nine and ten, and kept the aurora in view the whole of the time. I first saw it when passing Brixton church, then about nine o'clock; its appearance was that of a yellowish light, at a small altitude above the northern horizon. In the course of a few minutes, a few faint straggling streams glided upwards to a considerable height; and soon afterwards several groups of brilliant streaks of red and white light shot over an immense track of the northern heavens, to nearly the zenith. Besides these streamers, there were also splendid blushes of alternate stationary and moving red and white light. The sky was partially covered with thin vapoury clouds, which had an obvious influence on the colour, and the apparent horizontal motion of the light, which light also was easily distinguished to be behind or beyond these thin clouds of vapour; and assumed a deeper tinge of redness as the vapour became more dense between it and the spectator. As this was the first time of my observing this red light during the display of an aurora, I became very anxious to know its cause, for I never yet saw the electrical light in artificially attenuated air any thing like the colour of the light which I observed on this occasion. It was sometimes of a deep crimson, at other times of an almost fiery red, then pink, very light pink, next the white colour of the usual aurora, and so on for several alternate successions. And at other times the aurora would seem to reverse the order of colours, beginning with the ordinary white light, and passing through the different red tints down to the perfect crimson; and then return gradually to the ordinary white. I had several opportunities of observing these curious changes in the colour of the light before I arrived at Camberwell. Just before I entered the grove at Camberwell, then about half-past nine, the northern sky was illuminated through an immense horizontal range, with a

splendid red light, but when I arrived in the church yard, about five minutes afterwards, the red light had nearly disappeared, only a small portion remaining on the northern edge of a thin fleece of vapour, at a considerable altitude above the western horizon; being replaced by several splendid groups of the usual white streamers. From this time till a little before ten the aurora languished very considerably, but about five minutes before ten it re-appeared with all its former splendour, with the exception of the red colour. This last sudden display presented many exceedingly fine groups of intense streamers which shot upwards to the zenith, and covered an immense space in the heavens, but lasted only a few minutes before they vanished and appeared to leave the night in comparative darkness. I watched the aurora till about half-past ten, but as at that time there appeared no reason for its continuance much longer, I ceased my observations. I understand, however, that the aurora re-appeared in great splendour, and continued till three o'clock next morning.

I never, before, observed an aurora borealis expand to so great a horizontal range as that which I have now partly described. Lyra and Capella were excellently situated for giving a good idea of the horizontal extent of the aurora, the former star being just within its western, and the latter just within its eastern margin. The thin vapoury clouds presently clearing away, these two conspicuous stars were afterwards noticed to be within the limits of the auroral beams. Before ten o'clock the sky had become pretty clear, and the stars shone in every part of the visible heavens. I did not observe any meteoric stars.

WILLIAM STURGEON.

LXI.—*American Philosophical Society.*

Professor Bache, in behalf of Professor Alexander, of Princeton, made a verbal communication of a description of the aurora borealis, of September 3rd, 1839, as it appeared at Princeton,

At about ten or fifteen minutes past eight, P. M. an ill-defined, but considerably bright light was seen to extend for some distance above the horizon, in a direction nearly due east; it was similar, in intensity and appearance, to a lunar twilight. Soon after this, a continuous arch or zone of light was manifest, extending from the same spot to the opposite, or nearly opposite portion of the western horizon. This soon separated in two parts,* and, after a short interval, beams of

* Two arches, it is believed, were at this time formed, and either separated throughout their entire extent, or united only near their extremities; but this my notes do not explicitly state.

light shot up from the eastern portion of the arch which were speedily multiplied in every direction around the observer, except within about thirty degrees of the *true* (or, it might be, *magnetic*) south.

A corona was soon formed, which was at first quite indistinct, and was not continuous for any great length of time, during the existence of the aurora, except at the period of its greatest brilliancy. At about twenty minutes past eight, this corona was situated in a line with, and about midway between *• Aquilæ* and *• Lyræ*. This may be considered as a very tolerable approximation to its position, though, from the apparent intersection, or, as it might almost be termed, interweaving of the beams which composed it, it was not often easy to fix upon the place of its centre with much precision, if indeed that which seemed its centre, did not really change its place; since, at times, it seemed to occupy a position very sensibly lower than that which the preceding observation would indicate.

At about half past eight, the appearance of the aurora was superb. The radiations which extended from the corona, nearly reached the horizon in every direction, with the exception of those which tended toward the southern space before mentioned, which, it is believed, was even at this time bounded by something like an arch, that was convex towards the zenith. The aurora was often party-coloured; frequently of a rose-red, especially in spots, in that portion of the sky which might be supposed to be near the plane of the dipping needle; and also about the centre of the corona. It was in the part of the heavens here described, that the arch of greatest intensity could most commonly, if not uniformly, be traced: though the crown of it frequently faded away, or became excessively faint.

Between the spots, of red light, or beams of the same tint, others were observed, which, either from the effect of the first mentioned colour, or something peculiar to themselves, appeared of a colour approaching to a bottle-green.

At times, again, when the corona was deficient, the appearance of what remained on each side of the vacant spot, was not unlike that of two immense comets; their heads some small distance asunder, and their tails turned eastward and westward.

The light of the corona, when most perfect, was quite dense, not only at the central point, but also near to what seemed to be the outer limits of its radiations, at which the tint commonly exhibited the nearest approach to white.

Two meteors or shooting stars were seen, which in both

cases appeared to pass *between* the aurora and the eye of the observer; one nearly in the direction of the arch of greatest intensity, and the other almost perpendicular to it. The precise times of their appearance were not noted, though they fell within that period in which the phenomena already described were exhibited.

The corona formed again at nine; and, though again broken, was imperfectly visible after that time.

At half past nine, the eastern portion of the sky became tinted with intense red and green; but at half past ten, little else remained than the appearance of bright horizontal beams of white colour in the north.

If it be admitted that the centre of the aurora was precisely midway between α Aquilæ and α Lyræ, at twenty minutes past eight, its azimuth must have been $1^{\circ} 14' 42''$ E. of S., and its altitude $73^{\circ} 27' 6''$; the latitude of the observer being $49^{\circ} 20' 47''$ N. The point thus designated, would be very nearly in the direction of the dipping needle; the dip being, by observation, $72^{\circ} 47' 6''$ ($72^{\circ} 47.1'$) and the variation (though not accurately determined,) some 4° W. or that of the S. end of the needle, of course, the same extent to the east. The degrees of azimuth, reckoned on a parallel to the horizon at an altitude of 72° and more, being small, the deviation from the direction of the dipping needle, measured on the arc of a great circle, would be scarcely more than 1° towards the N. W.

Professor Bache stated that his own observations near Philadelphia, of the altitude of the apparent converging point of the auroral beams, at nine P. M. made it but about 69° . He had witnessed a case of the appearance of a dark spot of irregular shape, between two beams of light, which was certainly not a cloud, as the stars were not at all obscured by it, and which he supposed to be the phenomenon referred to recently by Professor Lloyd. No mottled clouds, such as usually attend the aurora, were visible during the period between nine and ten o'clock, when he had been able to observe. Professor Bache stated that he did not place much stress upon his measurements, as he had been prevented from sustained observation by indisposition. There had been, in the newspapers, an account of an auroral display visible at London, on the morning of the fourth of September, at about the same absolute time as at Princeton, according to Professor Alexander's observations. It was said to have been accompanied by a very unusual number of shooting stars, compared in one statement of the splendid display of November 13th, 1833.

LXII.—A New Method of Illuminating Microscopic Objects.

By the REV. J.B. READE, M.A., of Caius College, Cambridge.*

In Dr. Goring's valuable memoir of the Verification of Microscopic Phenomena, it is observed, "the verification of the real nature, form, and construction, of a vast variety of objects which elude the sense of touch by their extreme minuteness, can only be made out by an attentive study of their appearances, *under a variety of methods of illumination.*"† The methods of illumination at present adopted are four in number, and consist in the application of *direct* and *oblique reflected light*, and *direct* and *oblique transmitted light*.

The first two methods are applicable to opaque objects, but for the examination of transparent objects, all the methods are available. The two latter, however, it is well known, are those most commonly used.

Now, when microscopic objects, not opaque, are viewed with oblique reflected light—the flame of the candle being placed higher than the stage of the instrument, and its light condensed upon the object—it is invariably found that the maximum of condensed light which can be obtained by this method is sufficient for the full developement, of many important characters. If, again, transmitted light, either direct or oblique, be substituted for reflected light, obstacles of a still more serious nature greatly interfere with accurate investigation. Delicate tints are lost; colours naturally bright, or even brilliant, are all but absorbed; the texture and construction of objects are erroneously represented; and, in fact, nothing is seen, in many cases, but a magnified image of the object in mere black and white. Nor is this all; for besides this defective representation, the eye of the observer is always subject to much painful excitement, arising from the *intense illumination of the whole field of view*. And here, in fact, lies the great practical inconvenience of the present method; for, to take a common case—an object about 1-300th of an inch in diameter being placed in the middle of the field of view, the diameter of which is about 1-12th of an inch, and consequently being 1-625th part of the area of the field of view, the eye has to contend with 624 parts of the bright light, which are not brought to bear upon the illumination of the object. Hence, a method by which this intense glare shall be wholly removed, and that without the loss of a single effective ray, must evidently be superior to the one usually employed, in the ratio of at least 600 to 1.

* From Goring and Pritchard's *Micrographia*.

† *Microscopic Cabinet*, p. 183.

Being lately engaged in the examination of a few *test objects*, I happened to notice that the feathers of the *Lycæna argus*, when held above the flame of a candle, exhibited at a certain angle all their peculiar tints, and at the same time the flame was not visible to the eye. It then occurred to me, that by preserving the same angle under the microscope, the advantage of amplification would also be accompanied by the natural colours of the object. The requisite angle was readily obtained by making the axis of the microscope coincide with the line from the object to the eye, while the candle and the object retained their relative positions. The result accorded with my anticipation, and I was gratified by the exhibition of the most brilliant diamond tints, sparkling with exquisite lustre on a *jet black ground*. This new method of illuminating microscopic objects, it is at once apparent, consists in obtaining *oblique refracted light*.

On submitting a series of objects to the same illumination, I was soon convinced of the value of the discovery; and I scarcely know which to admire most—whether the very natural appearances of objects, adorned, as they invariably are, by the presence of their most delicate colouring, or the personal comfort of the observer, arising from the absence of all superfluous light. To illustrate the two methods by a reference to the telescope, it may be observed, that the discomfort of viewing spots on the sun not unaptly corresponds with the view of microscopic objects on an illuminated field; while the removal of all inconvenient and ineffective light from the field of the microscope corresponds with the clear and quiet view of stars on the dark blue vault of the firmament.

The most practicable mode of obtaining the illumination now described is to fix the object on the stage of the microscope, in the usual way, the axis of which must be inclined to the table, at about an angle of 45° , and then to place the candle about two inches below the stage, and about one or two inches to the right or left of it; but this lateral distance must be varied, according to the nature of the object and the angle of aperture of the instrument. It must be carefully borne in mind that the illumination will not be correct unless the field of view be *wholly darkened*.

To obtain this kind of illumination with facility and effect, it will be necessary to make some alterations in the construction of the instrument: as, for instance, in order to apply condensed light, the arm of the condenser must be placed in a ball-and-socket joint, or some similar contrivance must be adopted; for when it is perpendicular to the axis of the microscope, its introduction diverts the course of the rays from the candle to

the stage, and not unfrequently illuminates the field of view. The mirror also cannot be made available in its present position, for this kind of illumination, because light, when reflected from it, must of necessity illuminate the field. It must therefore be fixed on an extended and jointed arm; and when so constructed, microscopic objects may be viewed even in the daytime by oblique refracted light. Again, a very remarkable microscopic effect will be produced by giving a small vertical angular motion either to the body of the instrument or to the stage, as in Goring's Engiscope. By this means, the plane of the object which, owing to the present construction, is of necessity parallel to the diameter of the object-glass, may be inclined to it at different angles; and we shall thus obtain *oblique vision* as well as *oblique illumination*. These two conditions are absolutely necessary for obtaining, in many instances, the true effect of coloured objects even with the naked eye, and the introduction of magnifying powers between the object and the eye does not render these two conditions a whit the less necessary.

The effect of this new method of illumination may be tried with advantage on various subjects of the larger kind, as cuttings of wood, scales of fish, and wings of insects.* We may also apply it, with peculiar interest, to the investigation of the elementary organs of plants; animal tissues; mosses; corallines; crystals; and the scales of insects of the orders Lepidoptera and Thysanura. In each of all these some striking and hitherto unperceived character will be developed, and the observer will rise from his pursuit with a more thorough persuasion that the Being whose word is power, and by whom his own body "is fearfully and wonderfully made," has equally exhibited the matchless efforts of His skill in the exquisite polish of an insect's joints; in the opening of a leaf; and the pencilling of a flower.—To be a theoretical atheist is impossible.

Peckham, Nov. 1836.

* Among the various objects which shew the superiority of this kind of illumination over transmitted light, the spiral vessels of the hyacinth and the pollen of the convolvulus major are the most decided.—A. P.

LXIII. *The reason why a small pair of plates introduced into a circle of large pairs reduces the action of the whole battery to the same standard as if it were composed of all small pairs.* By W. H. HALSE, Esq. In a Letter to the Editor.

MR. EDITOR,—In the last number of "The Annals" I promised to explain my theory of the action of a small pair of plates when introduced into a circle of larger ones; as I considered that the theory commonly given as an explanation of it, was not founded on fact, viz., "that the positive electricity produced on the large plates more than that produced by the small plates, is instantly neutralized by an equal quantity of negative electricity, and that this extra quantity is continually being produced and as often neutralized."—Now it must be evident that this cannot be correct, for if it were so, double as much zinc must be dissolved from a four inch plate as there would be from a two inch plate, and twice the quantity of hydrogen gas evolved by the large copper. Experiment will prove that this is not the case, for in a compound circle consisting of twenty pairs of plates varying from one inch to eight inches in size, it will be found that the quantity of gas liberated from the coppers of the eight inch pairs will be no more than that evolved from the one inch pairs and the zinc dissolved is also equal. Although this theory is received by many persons as correct—and even by some lecturers on galvanism, for I have heard them give this explanation of it.—I am convinced that M. De la Rive did not mean it in this light, for he must undoubtedly have considered the extra productions and the extra re-compositions of the two electricities as due to chemical action on the plates beyond what was *necessary for the developement of the electric current circulating through unequal pairs*; the objection which I have advanced therefore can have no effect against his theory as he intended it to be understood, but only against that as received and advocated by those who are ignorant of his meaning. I, however, now give an explanation of its action according to my view of the subject and which I think will explain every effect.

The particles which compose the fluid contents of the battery contain both positive and negative electricity; the zinc contains both also; but the positive of the zinc and the negative of the fluid have an appendency to unite, and the particles of oxygen and zinc unite in consequence of it; therefore the negative electricity which was combined with the positive in the particle of zinc, remains on the mass of zinc and the positive which was combined with the negative in the particle of oxygen remains in the fluid, *thus the zinc plate contains an excess of negative and the liquid an excess of positive elec-*

tricity. The negative, therefore, proceeds from the zinc and the positive from the fluid, both travelling in different directions, viz.,—the positive from the fluid in contact with the zinc towards the copper in the same cell and the negative from the mass of zinc along the connecting wire to the copper in contact with it. Having thus laid the foundation of my theory, I will suppose a battery working, in which a pair of plates half the size of the others is introduced into the circle; say three pairs of simple circles united, and this small pair shall be the second pair. At the commencement of its action each pair of plates produces electricity according to its size, but only at the moment of uniting the two poles. The negative on the small zinc is conveyed by the connecting wire to the copper of the first pair, which at the same time attracts from the liquid in which it is immersed (viz. the first cell) an exact quantity of positive to neutralize itself, but as the liquid contains twice as much positive as is sufficient for the neutralization of the negative, the liquid of the first pair therefore remains positive. The negative electricity accumulated on the zinc of the third pair being twice as much as the positive contained in the liquid in which the small copper is immersed (second cell) and which copper is connected with this zinc, only one half of it escapes from the zinc by the connecting wire to this copper, and consequently the mass of zinc in this third cell remains negative, on account of there not being a sufficient quantity of positive to neutralize it.

Now as the zinc plates are positive with respect to the fluids in which they are immersed (that is that their positive electricity has an appetency to unite with the negative of the fluid) it shews that the action of the acid on the first and third pairs must be diminished. because, in number one, the fluid is positive by the accumulation of that kind as before stated, and as the zinc is naturally positive, and as two positives will not unite (supposing they were equal) it is evident that the chemical action on the zinc plate of number one must be diminished; the same in number three—the fluid is naturally negative, and the zinc naturally positive, (with respect to each other) but as there is an accumulation of negative electricity on the zinc of this pair as before stated, it prevents its associating with the negative oxygen of the fluid, so that the action is also diminished on this pair, but in the cell of the small pair the action goes on uninterruptedly, because there is no accumulation of either electricity on the zinc or in the fluid, therefore it will be perceived that the accumulation of the positive in the liquid of the first pair and of negative on the zinc of the third pair ACT A SIMILAR PART, AS A REGULATOR DOES TO A STEAM ENGINE, and in consequence of which there is no more

electricity produced from the large pairs (the commencement of the action excepted) than from the small pair; the greater the quantity of positive accumulated in the fluids and the more negative on the zincs, the less the amount of chemical action in these cells—It thus necessarily follows, that if this be correct, the prevailing opinion that the excess of either sort of electricity is immediately neutralized by its opposite must be incorrect, because no excess of either can be generated save in the first instance, but which is not neutralized but acts the purpose of regulating the action of the acid on the zinc—for example—supposing the large plates are ten inches square, and the small pair only one-fourth the size, and supposing that the large pair is capable of producing four times the quantity of electricity that the small pair will, then the action of the acid on one inch of the small pair will be as much as its action on four inches of the large pair or four times as much on an equal surface, that is, *that the same quantity of atoms both of oxygen and zinc will unite in the cell with the small pair, as will unite in the cell with the large pair*—the degree of action being regulated by this small pair, which has the effect for the above reasons of reducing the action of the battery to the same standard, as if it were composed of an equal number of these small plates, proving how necessary it is when we want large quantities of electricity and of a high tension, to be particular not to introduce a faulty pair into the circle. According to this theory, if there are five or ten pairs of plates increasing in size from one to ten inches, and the cells supplied with dilute acid sp. gr. 1068 (the zincs being all amalgamated) the hydrogen gas evolved from the ten inch coppers ought to be no more than that evolved by the one inch coppers in an equal time, and the quantity of zinc dissolved in each cell ought also to be equal. *Experiment will prove that such is the case.* I am well convinced that the above will be very difficult to be understood by the general reader, I therefore suggest the necessity of his placing before him a drawing of three simple circles united with each other, the centre one being half the size of the others, and I have no doubt that with a little attention—particularly to the first part of this letter,—that my theory will be fully comprehended, and that he will coincide with me in believing it to be the most plausible one of any that has as yet been advanced; still as it clashes with existing opinions—perhaps prejudices—of course I must expect the general attendants inseparable from innovators, viz. abuse and ridicule; but no matter, "*felix qui potuit rerum cognoscere causas.*"

In my last letter I also stated that I had discovered a plan to increase the intensity of the shocks ; I therefore now present you with the method :—

A method to increase the intensity of the Shock Apparatus.

In order to introduce the process more readily you will be pleased to place before you either a shock apparatus having two coils or else the sketch of one, (fig. 6. pl. IX.) and let the two screws connected with the terminations of the primary coil be marked No. 1 P. and No. 2 P. and let the two terminations of the secondary coil be marked No. 1 S. and No. 2 S., thus we shall have one side of the apparatus marked No. 1 P. and No. 1 S., and the other side, No. 2 P. and No. 2 S. I am thus particular in my explanations that I may be better understood by all your readers. Now let a wire pass from the screw No. 1 S. to No. 2 P. that they may be connected with each other. Next let a wire pass through the screw No. 1 P sufficiently long to be in contact with one pole of the battery and also in contact with one of the handles ; the other handle is to communicate with No. 2 S, whilst contact with the battery is broken by means of a wire connected with No. 2 P. Thus will the shock be considerably increased and about one half the wire now used for the secondary coil may be saved.

It will at first sight appear to the operator that the primary and secondary coils are connected with each other so as to form one continuous coil, and that the increase of the shock is gained on that account ; but with a little consideration it will be evident that this is not the case ; it is true that one end of the primary coil is connected with one end of the secondary coil, but in order that it should be the effect of one continuous coil, contact with the battery must be broken by means of a wire connected with No. 2 S instead of No. 2 P, and if this be done, it will be found that the shock is very trifling ; therefore it is clear that the increase of the shock is owing to some other cause which I think may be explained as follows :—

It is well known that a powerful shock can be obtained by the use of a primary wire alone ; but, that the shock is very considerably increased by coiling round it a secondary wire ; in that case however, this latter wire does not communicate either with the battery, or with the primary coil, the current giving the shock being what is termed “the induced current,” and which depends on the primary wire for its production,—the shock being regulated by the power of the primary coil, and also by the length of wire forming the secondary coil, but the shock *felt* certainly all proceeds from this latter coil, and the

shock which the primary coil was capable of giving, as certainly lost. Now it occurred to me that some means may be adopted to unite this latter shock with the secondary one, so that both may be brought into operation, and be made to pass through the body at the same time, and this is evidently effected by the before-mentioned means, for the induced current is obtained in an indirect manner, viz.: instead of the two extremities of this wire being in direct contact with the body as is the common method, only one end is in contact, whilst the other has to pass through the battery itself, and then comes to the body by means of the handle connected with the primary wire screw, No. 1, P; it is in this manner that the secondary current is obtained. The primary current is obtained by its passing from the battery through the screw No. 2, P, across to the screw No. 1, S, then circulating through the whole length of the secondary coil, and coming into contact with the body by the handle fastened to the screw No. 2, S, the other handle communicating with No. 1, P, and also with the battery; it will be thus perceived *that the increased effect is owing to the union of the primary current with the secondary current, and both being made to pass through the body at once.* With a sketch of a shock apparatus placed before you, I have no doubt my observations will be readily understood, for after the screws are marked and lines drawn to represent the wires, the whole will appear very plain. I have just constructed a small apparatus on this principle, having only four hundred feet of secondary wire, which is so powerful, that no one would be inclined to take the shock a second time, even with a single pair of half pint cylinders.

I remain, Mr. Editor,

Your obedient Servant,

W. H. HALSE.

Brent, near Ashburton.

LXIV.—*An Analysis of Mr. Harris's Investigation of Mr. Sturgeon's fourth Memoir.*

Dear Sir,—In my letter of 2nd December last, published in No. 22 of these *Annals*, (see page 332,) I promised to analyze your *excellent investigation* of my fourth Memoir; and now that I am about to enter on that important task, I must request your very patient attention, because, although I may have but little to do in this performance, I am desirous of doing that little well. Hence I shall not be enabled to gallop over your *excellent investigation* as fast as you have done over my memoir, nor shall I find time to follow your example in those *elegant exultations* in which you have so frequently enjoyed yourself. My

business, on this occasion, will be merely that of separating and classifying, the various materials of which your *investigation* is composed, and examining how far they are concerned in the great question at issue.

You commence your "investigation" in about the usual way of introduction; and if you had not perverted the meaning of my motives for submitting my fourth memoir to the "consideration of all the *learned* scientific bodies in Europe and America," and committed some other inaccuracies, which I will presently point out, your first paragraph might have been all very well. Now, instead of my saying that my memoir merited the consideration of those *learned* bodies, I have said, pretty clearly, that it "is *the subject of marine lightning conductors* which requires such general attention and rigorous investigation":* and had I denominated those scientific bodies to which I have submitted the consideration of my memoirs, *learned*, as you have done, I should have been guilty of an unpardonable insult to all other scientific bodies; because the adjective *scientific* implies *learned*; and I have a right to consider that *all scientific* bodies are *learned bodies*. Now I will not say that your perversion of my statement was *intentional*, I shall therefore place these two items under the head *Mistake*.

There are also other *mistakes* in your first compound paragraph; for I have nowhere said "that a metallic rod whilst transmitting a charge, is *always* productive of *powerful* lateral explosions;" nor have I anywhere said that "this effect, in the case of a lightning rod, is a very fearful circumstance." You must necessarily have observed, that the "fearful circumstances" which you mention are pointed out as applicable to *your own* lightning rod only; for it was that alone which I was investigating. Hence I find that your first paragraph is a compound of four pure *mistakes*.

Your 2nd paragraph is of no import, only as a connecting link between the first and third.

Paragraph 3 contains one *Mistake*: for I have nowhere "spoken in a slighting way" of you; but, on the contrary, I have always spoken of you in the most handsome manner. Read my memoir over again, and pay particular attention to paragraphs 215, 216, 217, and also my letter to you, dated September 12th, 1839, page 191 of this volume.

Hence you will find that, as a gentleman, I have paid every respect to you; but as to your experiments performed before the Navy Board, &c. you must necessarily permit that extent of criticism which is allowable on performances of so singular a character; and I must repeat that on this point I have done no

* See last paragraph of my letter to you, p. 192 of this volume.

more than to place those experiments in a proper light, in order that they may not have that *deceptive* influence on the readers of the "Annals of Electricity" as they probably had on the Navy Board, from a want of that explanation which, in my opinion, you ought *officially* to have given them.

Paragraph 4, is plain enough, and is a good epitome of some of Mr. Stephen Grey's discoveries; and will, no doubt, be very useful to those readers who were not previously aware of *copper being a conductor of electricity*: and that *glass is an insulator*. Further than this I cannot perceive any utility of paragraph 4: and certainly it contains nothing applicable to my experiments. I think it may properly be placed under the head *Neutral*.

Paragraph 5, is of a somewhat curiously complicated character. It is a compound of *mistakes, exultation, and a desire to lead your readers astray*. In this paragraph you are attempting to confound some of my experiments with those which you have described in paragraph 6, and as they have "really nothing" to do with each other, you are leading your readers from one of the items of the main subject; and as I have never made any "*claims to have*" my memoir "*considered by all the learned societies of Europe and America,*" you have, whilst exulting, tumbled into a, what? *Mistake*.

In paragraph 6, your statements are occasionally inconsistent with themselves, and even contradictory of each other. In one part you say, "as the charge accumulates on the inner surface, a corresponding quantity of electricity is forced off from the outer, and without this double effect takes place, we fail to accumulate a charge." And to *rivet* this statement firmly as a fact into our noddles; and "to render it evident" to our senses, you describe an illustrative experiment under the head (*e*). Now we have hardly had time to see this illustrative experiment, before you tell us it is all a deception; and that if we will read on through the long sentence (*f*) we shall find another *illustrative* experiment "which is sufficient to show, that the accumulated electricity is *never* exactly *balanced* between the opposite coatings, &c.,," so that one of these statements is an obvious contradiction of the other. You seem to have some vague idea respecting the condition of a charged jar: but you do not appear to be aware that the electric forces on the opposite sides of a charged jar may either be *equal* or *unequal* according to circumstances. I mention this fact with no other view than that of bringing it to *your* notice as a probable interesting novelty. The remainder of article 6 is an acknowledgment that a lateral spark does take place, with an attempt to give the phenomena

a different explanation to that given in my memoir: an explanation by no means new to the *partial* experiments which you have described, but perfectly inapplicable to my experiments, described in paragraph 202, of my fourth memoir, page 176 of this volume.

Paragraphs 7 and 8, are curious specimens of your mode of *reasoning* on philosophical subjects, and nothing more.

Paragraph 9 is simply an extract from my fourth memoir.

I will not take upon myself to say that, in paragraph 10, you have "laboured hard to invalidate my experiments," but you must permit me to point out an error or two into which you have fallen in *your version* of them.

I have not said that "a spark *is* felt at every discharge," but that "a pungent spark *would be* felt" if "the knuckle were to be presented to the conducting rod, &c."* Now, in consequence of this *little* mistake, (I hope it was not an intentional perversion) you have been led into a very serious error, by supposing that my experiments (I suppose you mean *results*) "could only be produced by continuing to work the machine in connexion with the jar," during the time that the discharges were made. If, instead of taking upon yourself to make such an assertion, you had condescended to *repeat* my experiments, you would soon have been convinced that there is no need of keeping the machine in motion, nor any necessity for keeping the jar in connexion with the wire conductor, in order to produce the lateral discharge which I have described.

Paragraph 11, is descriptive of experiments which have no bearing whatever on my memoir. The best epitome of them that I have seen is given by Cavallo, in the following note: "If a charged jar be insulated, and discharged with an insulating discharging rod, after the discharge both the sides of the jar, together with the discharging rod, will be found possessed of the electricity contrary to the electricity of that side of the jar which was touched last before the discharge: which shows that one side of a charged electric *may* contain a greater quantity of electricity than that, which is sufficient to balance the contrary electricity of the opposite side. This redundant electricity should be carefully considered in performing experiments of a *delicate* nature.

As, however, you may possibly wish me to notice a few items in paragraph 11, I will do so, and begin with the item (*p*): in which you, with a very proper motive, introduce your *unit* jar. Take a friend's advice on this instrument,

* See page 175 of this volume.

and never again depend on its indications as a *measurer of quantities* of the electric fluid, for it happens to be no measurer of either *quantity* or *intensity*. Let me endeavour to show you why. With a constant *surface* of the same coated glass, the intensity of the charge is *as the quantity deposited*, when the resistance is constant; but under *no other* circumstance. Now the resistance which a jar offers to the introduction of new or succeeding quantities, whilst charging, becomes gradually greater and greater as the charge advances, and consequently requires, from the unit jar, a continually *increasing* charge for every successive spark transmitted during the whole process of charging. Hence, you see, that *your* supposed *units* are continually varying: and *your* jar no measurer of electric action. You might have known this fact by placing the knob of a jar at a short distance from the prime conductor, where you would have seen that the sparks became less and less frequent as the charge advanced.

Items (*r*) and (*s*) are additional specimens of *your* mode of reasoning; and item (*t*) is not amiss. The two latter evince a *growing* propensity to *percussion* powder, but by no means any more applicable in this *investigation* than in your *illustration* of the effects of lightning on a ship's mast, by the wood-splitting apparatus. Your readers can have no doubt of your being eminently successful in the wood-splitting business after reading item (*s*): "whereas in passing the *slightest* spark, it (the percussion powder) inflames directly;" and they must necessarily acknowledge, that nothing less than the most penetrating mind could ever have assimilated the effects of "the slightest spark," with those of a flash of lightning, on a ship's mast.

Paragraph 12, is simply an epitome of *your* views of your described experiments.

Now comes paragraph 13 with all its singular misrepresentations, misinterpretations, &c., but I will notice one item only, because it is the clearest of the whole, and because by means of that item I shall be enabled to show you how easily you may be led into a mistake. I will premise, by acknowledging that Earl Stanhope was perfectly correct by supposing that a highly charged electric cloud would displace a portion of the earth's electric fluid; and that a *positively* charged cloud might thus cause a track of country beneath it, to become negatively electric; and so long as only *this* phenomenon occurred, your interpretation would be perfectly correct also; and there would be no need of any lightning conductor, because no lightning could possibly happen. But you know that lightning does occasionally happen: and you ought also to have known, that if a flash of

lightning from the supposed cloud Fig. 6, Plate VII, were to strike the supposed conductor *cc*, that neither that conductor nor the object beside it, would any longer be in the *same* electric condition that they were in *prior* to the discharge. The electric fluid constituting the flash would no longer be in the cloud, but would now be in the conductor *cc*, which would become highly *electro-positive* during the transit of fluid through the metal; and would thus cause electro-displacements, and consequent electric flashes amongst all those vicinal conducting bodies which were sufficiently near to each other to be within their respective spheres of action. Hence you see that *your* reasoning only applies to the state of the clouds and objects beneath them *prior* to the flash of lightning: and at a time when no conductor is needed: but my reasoning applies to the electric condition of a conductor whilst in the *absolute capacity of transmitting the lightning*. I hope you now perceive the distinction; as to the importance of our respective views I must leave you and others to judge.

In your 14th paragraph you obviously rely a great deal more upon the opinion of other persons than upon any knowledge of your own. I, on the contrary, do no such thing. I shall always venerate Priestly as a philosopher, but I must speak on points of doctrine according to my own experience; and although that eminent electrician did not observe the electrical effects of *lateral explosions* on vicinal bodies, I, and others, have observed them. And if *you* did not know of this fact before, I should advise you to read carefully my fifth memoir, which will very soon appear in these annals; and afterwards try to repeat some of the experiments which you will there find described: and in the mean time you may read the *Library of Useful Knowledge*: and if the facts stated there should happen to be "unfortunate for your *whole* doctrine," I hope that you will not blame me for it.

Paragraph 15 declares that you have no knowledge whatever of the "radiation of electric matter from conductors carrying the primitive discharge." Here again I must crave your indulgence not to blame me for your not being acquainted with this fact. I sincerely wish you had a better practical acquaintance with electrical phenomena than that which your *investigation* indicates, as it would have saved much trouble in bringing many common place facts to your notice. Item (*v*) is akin to item (*p*); and the *air electro-thermometer* on a par with the *unit jar*. Let me give you another piece of wholesome advice. Never again trust to an *electro-measuring apparatus*, until you have first become well acquainted with its *capabilities* and the *principles* of its action.

Paragraph 16 is a neutral: and paragraph 18 is simply a short extract: and paragraph 19 may be dismissed under the same head.

Paragraph 19, though mostly quotations, is a very important paragraph, because it is made the basis of all the unelectric matter of all the succeeding paragraphs in the *investigation*: and I am very glad that I am so near the end of it, for I am heartily tired of wading through such a mass of material as your *investigation* as composed of. Now, Sir, this unfortunately mischievous word *infinitely*, has given you a great deal of unnecessary trouble, for which I am exceedingly sorry. Had I said *immensely* instead of *infinitely*, I should have been more correct, and you would have had less trouble.

I do not find any thing further in your *investigation* excepting some trifling unelectro-ivective, which I have neither time nor inclination to notice. I sincerely wish you had been more serious and more scientific in your *investigation*, as it would then have been a pleasure to reason with you, and point out many other facts which bear upon the great question at issue.

I am, dear Sir,

Yours very truly,

WILLIAM STURGEON.

To W. Snow Harris, Esq.

ANNALS OF ELECTRICITY.

Fig. 3

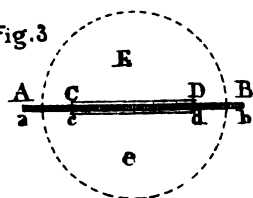


Fig. 1

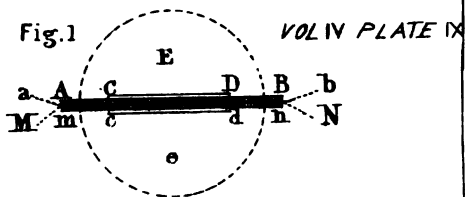


Fig. 2

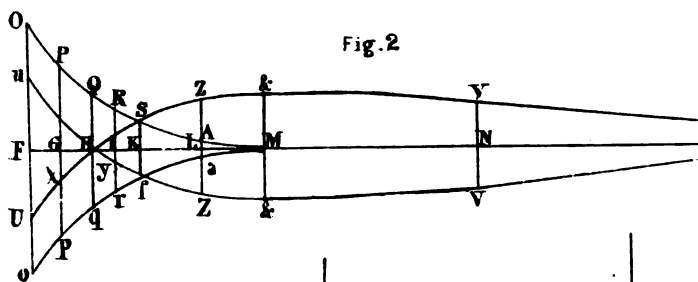


Fig. 6

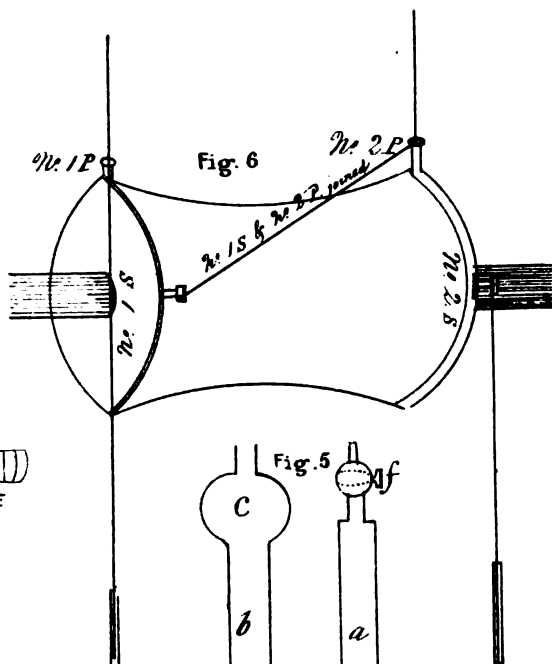


Fig. 5

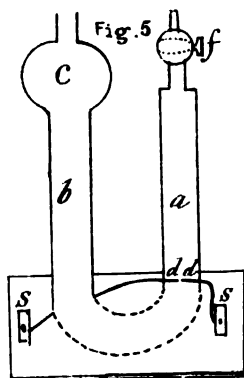
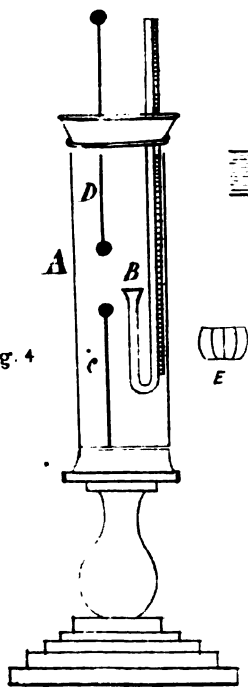


Fig. 4



THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

APRIL, 1840.

LXV.—*On the Connection between Electricity and Vegetation.*
By THOMAS PINE, Esq. (Resumed from page 253.)

If the air in a state of purity imparts a strong electric excitement to the embryos of plants, and thus produces a commencing movement in the vegetable juices, no sooner is the germ beginning to open than it craves the influence of vapours to maintain its increasing vitality, and promote its growth. Accordingly it is endowed with powerful conducting qualities in its structure and functions suited to the new element on which it has to act. The low springing herb, and the shooting and slightly expanding leaf of every description are now offered to our notice. For the fixed, rigid, texture of the seed and bud, is substituted the lithe, elongated, but acutely edged and pointed, form of the leaf, waving with every breeze, as if to catch and appropriate to its use every approaching vapour. These most intense attractors of electricity find it in the atmospheres of vapours as they gradually condense into the liquid state. In nothing is it more conspicuous than in the action of the tender herd upon the morning dew. The creeping species are the most remarkable for this quality, as they receive their watering in a great degree, particularly on fine summer mornings, from the dew which then appears to arise from the

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soil and to undergo a condensation from the attraction of the young leaves, aided probably by the cold produced by the evaporation. The down of the leaf, distinctly discernable only by the microscope, is in this case the principle organ of attraction, of which that of the *strawberry*, affords one of the most remarkable specimens. When in its growing state, its fine *needles*, placed at convenient distances from each other, exhibit at each point a transparent globule at a considerable distance from the surface of the leaf.

The same effect is observable as the general attendant of the settling of dew on the herbage, and on the early shooting of plants. Observing the dew on some herbs on which no down was apparent to the naked eye, I found on a minute microscopic inspection, that points of extreme tenuity were the agents in producing the watery effusion. This agency is peculiarly conspicuous in the *vegetable marrow*, the stem and leaves of which are every where bristling with fibrils, to whose acute extremities the globules attach. A beautiful display of the principle appears in the herb called *alpine wall cress*; the leaves of which are furnished with innumerable stems perpendicular to either surface, and branching out into four needles after the manner of our metallic protectors; at every point of which a globule attaches, while a larger globule is formed at their common centre. What but that peculiarly energetic property of electric attraction with which all vegetating points are endowed, can have operated to produce this effect? The condensing vapour exudes its latent imponderable fluid, which, entering the pores of the leaf, leaves a portion of water in the liquid state upon its surface; while a larger portion probably descends more completely deprived of its electric matter to the roots; thus furnishing an opportunity for its action from above in causing the absorption of the liquid forming the materials of the sap, through the exquisitely minute channels of the stem, from the innumerable storms at the ramifying extremities! A powerful attraction is thus manifested by each minute vegetable point for the floating vapours, which, operating in conjunction with a low temperature deprives them of their gaseous caloric, and reduces them to their liquid state. These points being exquisite conductors of electricity, and acting with peculiar energy, as such, upon the clouds and vapours of the atmosphere and at considerable distances, can it be questioned that this is the species of attraction that they have exerted in condensing the vapour, and attaching the liquid water to their extremities; that a portion of the subtle fluid has been imbibed by the leaf, and through the channels of the wood, which from their extreme minuteness may be considered as so many

*tubulated points** by which the electric influence is acting with the greatest possible efficacy on the rising sap. It is thus made to mount and spread, and impart vital energy and expansion to the whole plant. The same principle which acting from the pure air produces a first vegetable excitement on the prepared materials of the seeds and buds, and the commencing shoots, and which so evidently operates in proportion as it is increased by natural or artificial means in promoting this result, is now administered in much larger quantities through points of far greater number and efficacy, and conveys with it one of the most essential ingredients of nutrition; and thus the source of increasing vitality and an essential material of growth and vigour are furnished by the same process.

By a similar process, the loftier plants and more advanced vegetation receive a like vitalizing and nutritive influence from the clouds and vapours. The upper branches and spreading ramifications of the trees must act with great energy, and with little interruption from any contiguous bodies, on the clouded atmosphere which forms around them with every approaching shower. They are well known to be strong attracters of clouds; and it has been observed, that in insular or detached situations in which a few trees form the sole attracters, the atmosphere of vapour is in a great degree confined to their summits, and they have been described as so many "alembics" from whose leaves water is continually "distilled," and that "in some of the smaller islands of the West Indies, where there are no rivers or springs, the people are supplied with water merely by the dripping of large tall trees, which, standing in the bosom of a mountain, keep their heads constantly enveloped in fogs and clouds, from whence they dispense their kindly never-ceasing moisture."† But as I am acquainted with no facts which so distinctly manifest the

* Though *tubes* cannot in strictness be *points*, yet as these natural tubes bored by the divine hand with a minuteness precisely adapting them for the action of a fluid which was ascertained by the numerous experiments of the Abbé Nollet, to promote the flow of liquids through tubes, in degrees increasing with their minuteness, and as they approach by many degrees nearer to physical points than any that can be discerned by our unaided vision, I have used the above expression the more effectually to convey an idea of their conducting power. And when it is considered that the solid matter through which they are bored is a complete *non-conductor*, confining the action of the electricity entirely to the liquids contained in the tubes, they must, I think, be seen to be performing the functions of exquisite electrical points in promoting the rise and flow of the vegetable juices.

† White's "Natural History of Selborn."

agency of plants in depriving vapour of electricity, as those which appear in several experiments made by J. Williams, Esq. as related by him in his "Climate of Great Britain," I shall extract the particulars from his valuable work, to be inserted if it be thought requisite for the convenience of your readers, as an appendix to these remarks.

It is easy to see that several important consequences, highly beneficial to the animal as well as the vegetable system, must result from this arrangement. The attraction of the leaves must operate to produce the condensation of the vapours, at much higher temperatures, and in a more gentle and beneficial manner than could be effected by the mere action of cold; and as large quantities of the subtile fluid are thus gradually imbibed into the substance of plants, and in part transmitted to the earth, injurious accumulations of it in the atmosphere are averted. I conceive that in the absence of plants when there could be no cause of the condensation of vapours, except a low temperature, and were considerable quantities of aqueous gas actually formed in the atmosphere, the result must be, that no condensation would be effected till the cold had become extreme, when there would be a sudden immense percipitation of vapour, and its gaseous caloric being as suddenly set loose, a portion of it would rapidly combine with the particles of air, thus heating and rarifying it to an extraordinary degree, and another portion remaining in a state of separation from any gravitating matter would exhibit electrical phenomena in degrees which must greatly disturb the harmony of nature, and produce the most disastrous effects. There are some neglected or unproductive districts where an approach toward this condition of the elements has been experienced, of which I find the following general description. "In countries which are uncultivated the weather is generally in extremes. Rain when it falls takes the form of an overwhelming flood, not gently entering and moistening the soil, but rushing along the surface, tearing up one place, strewing another with *debris*, and reducing both to a state of indiscriminate ruin; while scarcely has the flood gone by, when the returning heat evaporates the little moisture which is left behind, and burns-up the coarse and scanty vegetation which the rains have fostered." Of the salutary change from such a state to one in which the extremes of cold and heat, of moisture and dryness, have been greatly mitigated with the progress of cultivation, an example is alleged from the central part of Scotland.* No particular

* "Library of Useful Knowledge" Vol. xv. Part I. pp. 2, 3. The

notice is here taken of the electrical phenomena attendant upon the other extremes, nor on the effects produced by an improved vegetation in softening their character; but it is well known, that intense heats and sudden depositions of water are the ordinary precursors and attendants of strokes of lightning. One of the heaviest and most sudden *sheets* of rain that has fallen under my observation, and indeed, I think it considerably exceeded any that I had previously experienced, was accompanied with a still more extraordinary display of lightning, the fluid flying with zig-zag course incessantly in all directions, and occasionally sweeping round and exhibiting to view the whole black concave whence it issued!—Storms and hurricanes are very much the attendants of a partial and irregular distribution of plants, such as is incident to uncultivated spots,* and in high latitudes where there is a large effusion of the solar beams;—but more especially over small, rich, cultivated clumps in islands surrounded by the ocean. The heavy accumulations of clouds, and rains on those spots, particularly when the winds set in a corresponding direction, are the well known accompaniments of those terrific electrical discharges which are occasionally witnessed in the islands of the western ocean. Since the temperature in those latitudes is seldom sufficiently reduced to be of much avail in the condensing process, the attraction of lofty and luxuriant trees is almost the sole instrument in producing the effect, and the immense quantities of caloric which are abstracted by evaporation from the ocean and plants united are concentrated on the devoted spots; the consequences are those prodigious discharges of water,

particulars of this change are as follows; “Within the experience of persons still living, the snow which in that country began to fall in November, was not wholly gone until the month of April; while in the middle of Summer the heat was so excessive, that agricultural labourers were obliged to suspend their toil during four or five hours in the middle of the day. At that time the autumnal rains frequently descended with so much violence, that the crops which had been retarded by the coldness of the spring, were prevented from ripening in the high grounds, were lodged and rotted in the lands that were lower, and swept away by the swelling of the streams over the holms and meadows. In the same spots at the present day, the quantity of snow which usually falls during the winter, is comparatively small, appears rarely before Christmas, and is gone in February or early in March. The summer heat is more uniformly distributed, seldom amounting to a degree oppressive to the labourer, or protracted to a term injurious to the crops, while the rain which follows is neither so violent in degree, nor so long continued, and happening when the grain is far advanced toward ripeness, the injury which it does is comparatively trifling.” *ibid.* pp. 3, 4.

* *Australia* is one of the uncultivated spots instanced under the above general description.

of heat, and of electricity which, are incident to those islands.

The co-operation of cold and of vegetation, in producing the condensation of vapours, or rather perhaps their respective effects in the absence of each other, forms of itself a curious and interesting subject of inquiry? When the sun's rays are so few and inefficient as to leave the ordinary temperature of the atmosphere considerably below the point at which vapour is transformed into water, they rarely if ever produce any electrical effects upon, or very near, the earth's surface. But when the temperature advances considerably above that point, there appears to be no other means of counteracting their tendency to float loosely in the atmosphere, and appear in the form of electricity in degrees which must prove highly injurious, but by their uniting with water in the form of vapour. This they do in the first instance from the ocean, seas, and rivers; but as they accumulate over particular spots, the agency of plants becomes more and more necessary in aiding the process, and in so appropriating and disposing of the vapours as to render them most effective both in carrying forward their own progressive advancement, and in protecting animals against the effect of extreme heat and pernicious accumulations of electricity. But a better opportunity will be afforded of treating of this subject in reviewing the state of vegetation in its most advanced stages.

It may deserve consideration how far the carbonic acid operates as a mean of exalting the conducting efficacy of plants, since they appear to live and thrive in proportion as a sufficient supply of this acid in its combination with water, is furnished to the roots. A sprig of mint, though peculiarly adapted to live and strike root in water, slightly impregnated with this gas, will speedily wither in water from which all access of this gas is excluded. If I might be allowed to conclude this paper with a conjecture concerning the superior conducting efficacy of vegetable points above those of metals and all other substances, and that notwithstanding the strong non-conducting property of their solids, it should be by a reference to the *expansive* nature of all growing substances, in which they greatly exceed even metals, together with the quantity of oxygen which is conveyed by means of the acid and water to the positive electricity from the upper extremities of the plant, while the remaining elements of carbon and hydrogen under the influence of negative electricity, combine to form the larger portion of the materials of the produce. The solid portion of the leaves is I believe known to be principally composed of those two elements, and their

attraction for the carbonic acid from the atmosphere, when it is decomposed, and yields pure oxygen gas under the influence of the sun's rays, while the carbon is retained, strongly favours the conclusion; and the ripening process of fruits, which seems to be in a great degree effected by the extraction of oxygen from their juices under the same influence, allowing those rays, to be positively electrical, tends to its confirmation. Hydrogen seems to be a promoter of the green colour of the leaves, as plants confined in this gas improve in greenness, and the discovery of the decomposition of water in vegetation by Mr. Weeks, enables us to perceive that hydrogen is supplied to plants from water, no less certainly than carbon from the carbonic acid. I apprehend that electricity will be allowed to be an essential agent in producing these decompositions. We are here reminded of a statement of Dr. Darwin, that "the production of oxygen gas from green leaves, and other green vegetable matter is probably owing to the decomposition of the water perspired by the plant, and as the oxygen may be expanded into gas by the sun's light, the hydrogen may be detained in the pores of the vegetables. Hence plants growing in the shade are white, and become green by being exposed to the sun's light, for their natural colour being blue, the addition of hydrogen adds yellow to the blue, and *turns* them green." It does indeed seem to be highly probable that as carbon and hydrogen, when separated from oxygen as they probably are in the most perfect manner in the leaves of plants, by the potent agency to which they are exposed from the solar rays, should unite in forming the substance and colour of the leaf. And to what but electricity or galvanism can we ascribe this twofold decomposition and recomposition in the production of the substance and beautiful compound colour which distinguishes the vegetable kingdom!

From Mr. Williams' "Climate of Great Britain," with Remarks.

His chapter on "the power of Vegetables to deprive Vapour of its Electricity," &c. abounds in evidence of the principles we are maintaining; but as his views in several respects do not coincide with the objects for which they are here adduced, I shall accompany the extracts with such observations as may throw some further light upon their principles.—He says p. 63 that "he was principally led to the consideration of this property in vegetables by remarking the drops of water on the

edges and angular points of the leaves of grass about sun-setting, before any general precipitation of nocturnal dew was perceptible; and from observing that trees and hedges occasioned a precipitation of fog, when attended with a *gentle wind, but not in a calm*.—"This important fact" he had "repeatedly verified" by the use of the electroscope. "Upon the 15th and 16th days of September, 1805," he writes "there was a very dense fog. On the morning of the 15th it was attended with a perfect calm: the trees and hedges being loaded with dew; but no precipitation of the fog, and the electricity strongly positive; at eight A. M. it began to clear away; and at ten A. M. the sun shone bright, and the day was tolerably fair. On the following morning the fog was equally dense; but about seven A. M. a gentle wind arose from the south, which bringing new particles of vapour within the conducting influence of trees and hedges, occasioned a copious fall of vapour from their leaves and small branches, but no general precipitation occurred." In general he observes that "if the electroscope shows signs of electricity it ceases to do so when brought within six or ten feet of a tree, or hedge, owing to the power these possess of drawing off the electricity of the vapour which comes within the power of attraction." This power may be rather referable to the general electricity of the atmosphere than of the vapours which are diffused through it, especially if no vapours are visible, since it may be questioned whether in a partially condensed state, or on their having been recently condensed by the conducting agency of plants a larger portion of the fluid may not float for a time in their vicinity than in situations in which the vapours remain in a state of transparent gas having no electrical atmosphere around them. But on the experiments on fogs we see a clear exemplification of the agency of vegetation, especially when assisted by winds, in causing the condensation of vapours to their liquid state at temperatures in which in its absence they would remain partially condensed yet floating in the atmosphere. It must also assist in causing a farther precipitation by keeping the temperature comparatively low in consequence of the quantities of the fluid it imbibes, and which would were the whole of what formed the caloric of vapour diffused through the atmosphere raise it to a temperature that would operate to prevent the process of precipitation from proceeding. In like manner it must avert dangerous accumulations of electricity, for a portion only of the fluid which is set loose by the previous cold, entering into combination with the particles of air, the remaining portion must float in an uncombined elec-

trical state, and animal life unprotected by the conducting agency of plants must be exposed to frequent attacks. What are the pestilential *simoons* of Arabia, but electric matter bursting from an atmosphere already charged with it, and which finding no medium of conveyance or opportunity of distribution, but through the bodies of animals thus exposed to its influence, produces effects upon the frame equalling or exceeding that of a powerful galvanic battery.

The following beautiful experiment clearly shews the existence of an electric atmosphere around the surface of vapour as it condenses by cooling, and that it is attracted and absorbed by the leaves of plants. "To the cap of a gold-leaf electroscope, I affixed a horizontal support for a candle, which projected two feet from the cap of the instrument placed near the edge of a table; on the floor immediately below was an earthen vessel containing hot water about one inch in depth; the candle being lighted, two or three red hot embers were dropped into the vessel of water, which instantly raised a sudden cloud of vapour; the electricity of this being collected by the candle connected with the electroscope, the gold leaf opened suddenly and struck the sides positively. Some branches of trees with their foliage were now placed between the vessel on the floor and the candle; the experiment being repeated the vapour passed through the interstices of the boughs but the electroscope opened only half an inch; more boughs were now added and slightly sprinkled with water to increase their conducting power, the experiment was again repeated; a great part of the vapour still made its way through the interstices of the leaves and branches, but so completely deprived of its electricity that the gold leaf did not diverge in the smallest degree."—pp. 73, 74—the effect of the sprinkling of the water was probably to aid a little in the condensation by its cooling influence, and thus to add rather to the quantity of electric matter discharged than by its far inferior conducting agency to that of living leaves to aid in its removal. The great efficacy of the leaves in absorbing the fluid is thus rendered strikingly apparent; the continuance of a great portion of the vapour in an uncondensed state must have been the consequent of the raised temperature; in nature temperature and vegetable attraction are often so admirably arranged that the vapour disappears, and a clear, mild, and moderately electrified atmosphere is the frequent result of their cooperation.

On the several species of vapours.

It may not be improper here to observe that there appears to be a threefold provision in nature for the irrigation of plants of different magnitudes and under different circumstances. The clouds which sail aloft are the evident result of the action of the solar beams upon the waters ; and these are much devoted to the larger and loftier vegetation. After a drying day the creeping species and the forming shoots of plants require immediate moisture, and it accumulates upon them in the absence of the sun in the form of dews, the sources of which have been differently ascribed either to a concentration of the cooling atmosphere, or to a rise of vapour from the soil in a similar state of cooling. My own observations by glasses inverted upon the soil during the night season have been altogether in favour of the latter conclusion. They are uniformly wetted *internally* in quantities proportioned to the dew which appears upon the herbage, and its disappearance is accompanied with that of the wetness in the glasses even though they remain inverted on the soil. It is indeed a curious circumstance which I have proved by repeated observations that in a clear still atmosphere in the sun's absence when dew is forming, a corresponding wetness appears on the internal surface of any vessel whether of glass or earthenware which is inverted on the soil ; and on the other hand when there is an opposite tendency in the atmosphere, when it inclines to the formation of clouds and rain, an opposite effect will appear in the inverted vessel, no moisture will arise in it from the soil, and if any had been previously formed in it, or is produced, as by breathing, it will presently disappear. Are not these phænomena ascribable to variations in the relative electricity in the soil and the atmosphere, co-operating probably with corresponding changes of temperature ? In the sun's absence no evaporation proceeds from plants, the atmosphere cools and becomes less impregnated with electric matter, the soil retains a larger proportion of warmth and electricity it had imbibed in the preceding day, than the atmosphere and the moisture which by the sun's rays is drawn through the leaves, remaining also in the soil, combines with the warmth and electricity to form a rising vapour ; the evaporation produces a coolness on the surface of the ground, which co-operating with the strong attraction of the young and almost exhausted germs and herbage condenses the vapour, and thus a seasonable supply of electricity and moisture with an improved temperature is ad-

ministered to these minute and tender productions. This appears to be the case when the atmosphere clears in the sun's absence and a transparent sky is seen, the particles of electricity and of moisture in its combining into pure aqueous gas, and leaving it in a dry and comparatively negative state ; but when an opposite tendency prevails and the atmosphere becomes suffused with clouds and vapours, the caloric and electricity of these vapours being released from an opposite relation to the soil which is now comparatively cool and negatively electrical, and consequently in a condition to *absorb* the moisture and electricity of the condensing vapour. This condition of the soil imparts a corresponding state to the plants which extends to their upper extremities, and hence a general disposition both in the soil and plants to imbibe the combined vitality and nutriment which is thus imparted to them.

Fogs appear to have a somewhat different origin from either clouds or dews. They are chiefly prevalent in the winter and late autumn seasons ; and seem to result from a condensation in the lower regions of the atmosphere by the action of cold in the sun's absence, or in part from the imperfect action of his few and languid rays in raising vapour from a previously moistened soil. The plants then in a state of vegetation being evergreen shrubs and quickset hedges, chiefly, the moisture attaches to them in large quantities, and seems to furnish them with a supply of nutriment in its union with electric matter that greatly conduces to their support and progress, at seasons when the more active species are nearly dormant.

LXVI. *Extract from the Instructions for the Scientific Expedition to the Antarctic Regions, prepared by the President and Council of the Royal Society*.*

PHYSICS AND METEOROLOGY.

The council of the royal society are very strongly impressed with the number and importance of the desiderata in

* The President and Council having been informed by the Lords Commissioners of the Admiralty that it had been determined, in conformity with their recommendation, to send out captain James C. Ross on an Antarctic Expedition for scientific objects, and having been requested to communicate any suggestions upon subjects to which they might wish his attention to be called, referred the consideration of each to distinct Committees, namely, those of Physics, Meteorology, Geology, Botany, and Zoology, the result of whose labours is the Report from which the above is an extract.—ED.

physical and meteorological science, which may wholly or in part be supplied by observations made under such highly favourable and encouraging circumstances as those afforded by the liberality of her majesty's government on this occasion. While they wish therefore to omit nothing in their enumeration of those objects which appear to them deserving of attentive inquiry on sound scientific grounds, and from which consequences may be drawn of real importance, either for the settlement of disputed questions, or for the advancement of knowledge in any of its branches,—they deem it equally their duty to omit or pass lightly over several points which, although not without a certain degree of interest, may yet be regarded in the present state of science rather as matters of abstract curiosity than as affording data for strict reasoning; as well as others, which may be equally well or better elucidated by inquiries instituted at home and at leisure.

TERRESTRIAL MAGNETISM.

The subject of most importance, beyond all question, to which the attention of Captain James Clark Ross and his officers can be turned,—and that which must be considered as, in an emphatic manner, the great scientific object of the expedition,—is that of Terrestrial Magnetism; and this will be considered: 1st, as regards those accessions to our knowledge which may be supplied by observations to be made during the progress of the expedition, independently of any concert with or co-operation of other observers; and 2ndly, as regards those which depend on and require such concert; and are therefore to be considered with reference to the observations about to be carried on simultaneously in the fixed magnetic observatories, ordered to be established by Her Majesty's Government with this especial view, and in the other similar observatories, both public and private, in Europe, India, and elsewhere, with which it is intended to open and maintain a correspondence.

Now it may be observed, that these two classes of observations naturally refer themselves to two chief branches into which the science of terrestrial magnetism in its present state subdivides itself, and which bear a certain analogy to the theories of the elliptic movements of the planets, and of their periodical and secular perturbations. The first comprehends the actual distribution of magnetic influence over the globe, at the present epoch, in its mean to average state, when the effects of temporary fluctuations are either neglected or eliminated by extending the observations over a sufficient time to

neutralize their effects. The other comprises the history of all that is not permanent in the phenomena, whether it appear in the form of momentary, daily, monthly, or annual change and restoration, or in progressive changes not compensated by counter changes, but going on continually accumulating in one direction, so as in the course of many years to alter the mean amount of the quantities observed. These last-mentioned changes hold the same place, in the analogy alluded to, with respect to the mean quantities and temporary fluctuations, that the secular variations in the planetary movements must be regarded as holding, with respect to their mean orbits on the one hand, and their perturbations of brief period on the other.

There is, however, this difference, that in the planetary theory all these varieties of effect have been satisfactorily traced up to a single cause, whereas in that of terrestrial magnetism this is so far from being demonstrably the case, that the contrary is not destitute of considerable probability. In fact, the great features of the magnetic curves, and their general displacements and changes of form over the whole surface of the earth, would seem to be the result of causes acting in the interior of the earth, and pervading its whole mass; while the annual and diurnal variations of the needle, with their train of subordinate periodical movements, may, and very probably do arise from, and correspond to electric currents produced by periodical variations of temperature at its surface, due to the sun's position above the horizon, or in the ecliptic, modified by local causes; while local or temporary electric discharges, due to the thermic, chemical, or mechanical causes, acting in the higher regions of the atmosphere, and relieving themselves irregularly or at intervals, may serve to render account of those unceasing, as they seem to us casual movements, which recent observations have placed in so conspicuous and interesting a light. The electrodynamic theory, which refers all magnetism to electric currents, is silent as to the causes of those currents, which may be various, and which only the analysis of their effects can teach us to regard as internal, superficial, or atmospheric.

It is not merely for the use of navigators that charts, giving a general view of the lines of magnetic declination, inclination, and intensity, are necessary. Such charts, could they really be depended on, and where they in any degree complete, would be of the most eminent use to the theoretical inquirer, not only as general directions in the choice of empirical formulæ, but as powerful instruments for facilitating numerical investigation, by the choice they afford of data

favourably arranged; and above all, as affording decidedly the best means of comparing any given theory with observation. In fact, upon the whole, the readiest, and beyond comparison the fairest and most effectual mode of testing the numerical applicability of a theory of terrestrial magnetism, would be, not servilely to calculate its results for given localities, however numerous, and thereby load its apparent errors with the real errors, both of observation and local magnetism, but to compare the totality of the lines in our charts with the corresponding lines, as they result from the formulæ to be tested, when their general agreement or disagreement will not only show how far the latter truly represent the facts, but will furnish distinct indications of the modifications they require.

Unfortunately for the progress of our theories, however, we are yet very far from possessing charts even of that one element, the Declination, most useful to the navigator, which satisfy these requisites; while as respects the others (the Inclination and Intensity) the most lamentable deficiencies occur, especially in the Antarctic regions. To make good these deficiencies by the continual practice of every mode of observation appropriate to the circumstances in which the observer is placed throughout the voyage, will be one of the great objects to which attention must be directed. And first—

At sea.—We are not to expect from magnetic observations made at sea the precision of which they are susceptible on land. Nevertheless, it has been ascertained that not only the Declination, but the Inclination and Intensity can be observed, in moderate circumstances of weather and sea, with sufficient correctness, to afford most useful and valuable information, if patience be bestowed, and proper precautions adopted. The total intensity, it is ascertained, can be measured with some considerable degree of certainty by the adoption of a statical method of observation recently devised by Mr. Fox, whose instrument will be a part of the apparatus provided. And when it is recollected that but for such observations the whole of that part of the globe which is covered by the ocean must remain for ever a blank in our charts, it will be needless further to insist on the necessity of making a daily series of magnetic observations, in all the three particulars above-mentioned, whenever weather and sea will permit, an essential feature in the business of the voyage, in both ships. Magnetic observations at sea will, of course be affected by the ship's magnetism, and this must be eliminated to obtain results of any service. To this end,

First. Every series of observations made on board should be accompanied with a notice of the direction by compass of the ship's head at the time.

Secondly. Previous to sailing, a very careful series of the apparent deviations, as shown by two compasses permanently fixed, (the one as usual, the other in a convenient position, considerably more forward in the ship,) in every position of the ship's head, as compared with the real position of the ship, should be made and recorded, with a view to attempt procuring the constants of the ship's action according to M. Poisson's theory*; and this process should be repeated on one or more convenient occasions during the voyage; and, generally, while at anchor, every opportunity should be taken of swinging round the ship's head to the four cardinal points, and executing in each position a complete series of the usual observations.

Thirdly. Wherever magnetic instruments are landed and observations made on *terra firma*, or on ice, the opportunity should be seized of going through the regular series on ship-board with more than usual diligence and care, so as to establish by actual experiment in the only unexceptionable manner the nature and amount of the corrections due to the ship's action for that particular geographical position, and by the assemblage of all such observations to afford data for concluding them in general.

Fourthly. No change possible to be avoided should be made in the disposition of considerable masses of iron in the ships during the whole voyage; but if such change be necessary, it should be noted.

Fifthly. When crossing the magnetic line of no dip it would be desirable to go through the observations for the dip with the instrument successively placed in a series of different magnetic azimuths, by which the influence of the ship's magnetism in a vertical direction will be placed in evidence.

On land, or on ice.—As the completeness and excellence of the instruments with which the Expedition will be furnished will authorise the utmost confidence in the results obtained by Captain Ross's well known scrupulosity and exactness in their use, the redetermination of the magnetic elements at points where they are already considered as ascertained, will be scarcely less desirable than their original determination at stations where they have never before been observed. This is the more to be insisted on, as lapse of time changes these

* See Appendix A.

elements in some cases with considerable rapidity ; and it is therefore of great consequence that observations to be compared should be as nearly contemporary as possible, and that data should be obtained for eliminating the effects of secular variations during short intervals of time, so as to enable us to reduce the observations of a series to a common epoch.

On the other hand it cannot be too strongly recommended, studiously to seek every opportunity of landing on points (magnetically speaking) unknown, and determining the elements of those points with all possible precision. Nor should it be neglected, whenever the slightest room for doubt subsists, to determine at the same time the geographical position of the stations of observation in latitude and longitude. When the observations are made on ice, it is needless to remark that this will be universally necessary.

With this general recommendation it will be unnecessary to enumerate particular localities. In fact, it is impossible to accumulate too many. Nor can it be doubted that in the course of antarctic exploration, many hitherto undiscovered points of land will be encountered, each of which will, of course, become available as a magnetic station, according to its accessibility and convenience.

There are certain points in the regions about to be traversed in this voyage which offer great and especial interest in a magnetic point of view. These are, first, the south magnetic pole (or poles), intending thereby the point or points in which the horizontal intensity vanishes and the needle tends virtically downwards ; and secondly, the points of maximum intensity, which, to prevent the confusion arising from a double use of the word poles, we may provisionally term magnetic *foci*.

It is not to be supposed that Captain Ross, having already signalized himself by attaining the northern magnetic pole, should require any exhortation to induce him to use his endeavours to reach the southern. On the contrary, it might better become us to suggest for his consideration, that no scientific datum of this description, nor any attempt to attain very high southern latitudes, can be deemed important enough to be made a ground for exposing to *extraordinary* risk the lives of brave and valuable men. The magnetic pole, though not attained will yet be pointed to by distinct and unequivocal indications ; viz. by the approximation of the dip to 90° ; and by the convergence of the magnetic meridians on all sides towards it. If such convergence be observed over any considerable region, the place of the pole may hence be deduced, though its locality may be inaccessible.

M. Gauss, from theoretical considerations, has recently

assigned a probable position in lon. 146° E., lat. 66° S., to the southern magnetic pole, denying the existence of two poles of the same name, in either hemisphere, which, as he justly remarks, would entail the necessity of admitting also a third point, having some of the chief characters of such a pole intermediate between them. That this is so, may be made obvious without following out his somewhat intricate demonstration, by simply considering, that if a needle be transported from one such pole to another of the same name, it will *begin* to deviate from perpendicularity *towards* the pole it has quitted, and will end in attaining perpendicularity again, after pointing in the latter part of its progress obliquely *towards the pole to which it is moving*, a sequence of things impossible without an intermediate passage through the perpendicular direction.

It is not improbable that the point indicated by M. Gauss will prove accessible; at all events it cannot but be approachable sufficiently near to test by the convergence of meridians the truth of the indication; and as his theory gives within very moderate limits of error the true place of the northern pole, and otherwise represents the magnetic elements in every explored region with considerable approximation, it is but reasonable to recommend this as a distinct point to be decided in Captain Ross's voyages. Should the decision be in the negative, i. e. should none of the indications characterizing the near vicinity of the magnetic pole occur in that region, it will be to be sought; and a knowledge of its real locality will be one of the distinct scientific results which may be confidently hoped from this Expedition, and which can only be attained by circumnavigating the antarctic pole compass in hand.

The actual attainment of a *focus* of maximum intensity is rendered difficult by the want of some distinct character by which it can be known, previous to trial, in which direction to proceed, when after increasing to a certain point the intensity begins again to diminish. The best rule to be given, would be (supposing circumstances would permit it) on perceiving the intensity to have become nearly stationary in its amount, to turn short and pursue a course at right angles to that just before followed, when a change could not fail to occur, and indicate by its direction towards which side the focus in question were situated.

Another, and as it would appear, a better mode of conducting such a research, would be, when in the presumed neighbourhood of a focus of maximum intensity, to run down two parallels of latitude or two arcs of meridians separated by an interval of moderate extent, observing all the way in each, by which observations, when compared, the con-

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cavities of the isodynamic lines would become apparent, and perpendiculars to the chords, intersecting in or near the foci, might be drawn.

Two foci or points of maximum *total* intensity are indicated by the general course of the lines in Major Sabine's chart in the Southern Hemisphere, one about long. 140° E., lat. 47° S., the other more obscurely in long. 235° E., lat. 60° S., or thereabouts. Both these points are certainly accessible; and as the course of the Expedition will lead not far from each of them, they might be visited with advantage by a course calculated to lead directly across the isodynamic ovals surrounding them.

Pursuing the course of the isodynamic lines in the chart above mentioned, it appears that one of the two points of *minimum* total intensity, which must exist, if that chart be correct, may be looked for nearly about lat. 25° S., long. 12° W., and that the intensity at that point is probably the least which occurs over the whole globe. Now this point does not lie much out of the direct course usually pursued by vessels going to the Cape. It would therefore appear desirable to pass directly over it, were it only for the sake of determining by direct measure the least magnetic intensity at present existing on the earth, an element not unlikely to prove of importance in the further progress of theoretical investigation, Excellent opportunities will be afforded for the investigation of all these points, and for making out the true form of the isodynamic ovals of the South Atlantic, both in beating up for St. Helena, and in the passage from thence to the Cape; in the course of which, the point of least intensity will, almost of necessity, have to be crossed, or at least approached very near.

Nor is the theoretical line indicated by Gauss as dividing the northern and southern regions, in which free magnetism may be regarded as superficially distributed, undeserving of attention. That line cuts the equator in 6° east longitude, being inclined thereto (supposing it a great circle) 15° , by which quantity it recedes from the equator northward in going towards the west of the point of intersection. Observations made at points lying in the course of this line may hereafter prove to possess a value not at present contemplated.

As a theoretical datum, the horizontal intensity has been recommended by Gauss, in preference to the total, not only as being concluded from observations susceptible of great precision, but as affording immediate facilities for calculation. As it cannot now be long before the desideratum of a chart of the horizontal intensity is supplied, the maxima and minima of this element may also deserve especial inquiry, and may be ascertained in the manner above pointed out.

The maxima of horizontal intensity are at present undetermined by any direct observation. They must of necessity, however, lie in lower magnetic latitudes than those of the total intensity, as its minima must in higher; and from such imperfect means as we have of judging, the conjectural situations of the maxima may be stated as occurring in

20° N.	80° E.	I.
7 N.	260 E.	II.
3 S.	130 E.	III.
10 S.	180 E.	IV.

Observations have been made of the horizontal intensity in the vicinities of II, and III., and are decidedly the highest which have been observed anywhere.

In general, in the choice of stations for determining the absolute values of the three magnetic elements, it should be borne in mind, that the value of each new station is directly proportional to its remoteness from those already known. Should any doubt arise, therefore, as to the greater or less eligibility of particular points, a reference to the existing magnetic maps and charts, by showing where the known points of observation are most sparingly distributed, will decide it.

For such magnetic determinations as those above contemplated, the instruments hitherto in ordinary use, with the addition of Mr. Fox's apparatus for the statical determination of the intensity, will suffice; the number of the sea observations compensating for their possible want of exactness. The determinations which belong to the second branch of our subject,—viz. those of the diurnal and other periodical variations, and of the momentary fluctuations of the magnetic forces,—require, in the present state of our knowledge, the use of those more refined instruments recently introduced. Being comparative rather than absolute, they depend in great measure (and as regards the momentary changes, wholly) on combined and simultaneous observation.

The variations to which the earth's magnetic force is subject, at a given place, may be classed under three heads, namely, 1. the *irregular* variations, or those which *apparently* observe no law; 2. the *periodical* variations whose amount is a function of the *hour* of the day, or of the *season* of the year; and, 3. the *secular* variations, which are either slowly progressive, or else return to their former values in periods of very great and unknown magnitude.

The recent discoveries connected with the *irregular* variations of the magnetic declination, have given to this class of changes a prominent interest. In the year 1818 M. Arago

made, at the Observatory of Paris, a valuable and extensive series of observations on the declination changes; and M. Kupffer having about the same time undertaken a similar research at Cazan, a comparison of the results led to the discovery that the perturbations of the needle were *synchronous* at the two places, although these places differed from one another by more than forty-seven degrees of longitude. This seems to have been the first recognition of a phenomenon, which now, in the hands of Gauss and those who are labouring with him, appears likely to receive a full elucidation.

To pursue this phenomenon successfully, and to promote in other directions the theory of terrestrial magnetism, it was necessary to extend and vary the stations of observation, and to adopt at all a common plan. Such a system of simultaneous observations was organized by Von Humboldt in the year 1827. Magnetic stations were established at Berlin and Freyberg; and the Imperial Academy of Russia entering with zeal into the project, the chain of stations was carried over the whole of that colossal empire. Magnetic *houses* were erected at Petersburg and at Cazan; and magnetic instruments were placed, and regular observations commenced, at Moscow, at Sitka, at Nicolajeff in the Crimea, at Barnaoul and Nertschinsk in Siberia, and even at Peking. The plan of observation was definitely organized in 1830; and simultaneous observations were made seven times in the year, at intervals of an hour for the space of forty-four hours.

In 1834 the illustrious Gauss turned his attention to the subject of terrestrial magnetism; and having contrived instruments which were capable of yielding results of an accuracy before unthought of in magnetic researches, he proceeded to inquire into the simultaneous movements of the horizontal needle at distant places. At the very outset of his inquiry he discovered the fact, that the synchronism of the perturbations was not confined (as had been hitherto imagined) to the larger and extraordinary changes; but that even the minutest deviation at one place of observation had its counterpart at the other. Gauss was thus led to organize a plan of simultaneous observations, not at intervals of an hour, but at the short intervals of five minutes. These were carried on through twenty-four hours six* times in the year; and magnetic stations taking part in the system were established at

* Recently reduced to *four*.

Altona, Augsburg, Berlin, Bonn, Brunswick, Breda, Breslau, Cassel, Copenhagen, Dublin, Freyberg, Gottingen, Greenwich, Halle, Kazan, Cracow, Leipsic, Milan, Marburg, Munich, Naples, St. Petersburg, and Upsala.

Extensive as this plan appears, there is much yet remaining to be accomplished. The stations, numerous as they are, embrace but a small portion of the earth's surface; and what is of yet more importance, none of them are situated in the neighbourhood of those *singular points* or curves on the earth's surface, where the *magnitude* of the changes may be expected to be excessive, and perhaps even their *direction* inverted. In short, a wider system of observation is required to determine whether the amount of the changes (which is found to be very different in different places) is dependent simply on the *geographical* or on the *magnetic* co-ordinates of the place; whether, in fact, the variation in that amount be due to the greater or less distance of a disturbing centre, or to the modifying effect of the mean magnetic force of the place, or to both causes acting conjointly. In another respect also, the plan of the simultaneous observations admits of a greater extension. Until lately the movements observed have been only those of the magnetic *declination*, although there can be no doubt that the *inclination* and the *intensity* are subject to similar perturbations. Recently, at many of the German stations, the *horizontal component* of the intensity has been observed, as well as the declination; but the determination of another element is yet required, before we are possessed of all the data necessary in this most interesting research.

The magnetic observations about to be established in the British Colonies, by the liberality of the Government, will (it is hoped) supply in a great measure these desiderata. The stations are widely scattered over the earth's surface, and are situated at points of prominent interest with regard to the Isodynamic and Isoclinal lines. The point of maximum intensity in the northern hemisphere is *in* Canada; the corresponding maximum in the southern hemisphere is *near* Van Diemen's Land; St. Helena is close to the line of *minimum intensity*; and the Cape of Good Hope is of importance on account of its southern latitude. At each observatory the changes of the *vertical component* of the magnetic force will be observed, as well as those of the *horizontal component* and *declination*; and the variations of the two components of the force being known, those of the *inclination* and of the *force* itself are readily deduced. The simultaneous observations of these three elements will be made at numerous and stated periods, and we have every reason to hope that the

directors of the various European observatories will take part in the combined system.

But interesting as these phenomena are, they form but a small part of the proper business of an observatory. The *regular* changes (both periodic and secular) are no less important than the irregular; and they are certainly those by which a patient inductive inquirer would seek to ascend to general laws. Even the empirical expression of these laws cannot fail to be of the utmost value, as furnishing a correction to the absolute values of the magnetic elements, and thereby reducing them to their mean amount.

The hourly changes of the *declination* have been frequently and attentively observed; but with respect to the periodical variations of the other two elements, our information is as yet very scanty. The determination of these variations will form an important part of the duty of the magnetic observatories; and from the accuracy of which the observations are susceptible, and the extent which it is proposed to give them, there can be no doubt that a very exact knowledge of the empirical laws will be the result.

With respect to the *secular* variations, it might perhaps be doubted whether the limited time during which the observatories will be in operation is adequate to their determination. But it should be kept in mind that the monthly mean corresponding to each hour of observation will furnish a separate result; and that the number and accuracy of the results thus obtained may be such, as fully to compensate for the shortness of the interval through which they are followed. A beautiful example of such a result, deduced from three years' observation of the declination, is to be found in the first volume of Gauss's magnetical work, of which a translation is published in the fifth number of Taylor's Scientific Memoirs.

It remains to say a few words of the instrumental means which have been adopted for the attainment of these ends.

The magnetic instruments belonging to each observatory and in constant use, are, 1. a declination instrument; 2. a horizontal force magnetometer; 3. a vertical force magnetometer. These instruments are constructed after the plan adopted by Professor Lloyd in the Magnetic Observatory of Dublin. The magnet, in the two former, is a heavy bar, fifteen inches long, and upwards of a pound in weight. In the declination instrument the magnet rests in the magnetic meridian, being suspended by fibres of silk without torsion. In the horizontal force magnetometer, the magnet is supported by two parallel wires, and maintained in a position at right angles to the magnetic meridian by the torsion of their upper extremities. In

both instruments the changes of position of the magnet are read off by means of an attached collimator having a divided scale in its focus. The magnetometer for the vertical force is a bar resting by knife edges on agate planes, and capable of motion therefore in the vertical plane only. This bar is loaded, so as to rest in the horizontal position in the mean state of the force; and the deviations from that position are read off by micrometers near the two extremities of the bar.

In addition to these instruments, each observatory is furnished with a dip circle, a transit with an azimuth circle, and two chronometers. Each vessel also is supplied with a similar equipment. Should therefore the ships be under the necessity of wintering in the ice,—and generally, on every occasion when the nature of the service may render it necessary to pass a considerable interval of time in any port or anchorage,—the magnetometers should be established, and observations made with all the regularity of one of the fixed observatories, and with strict attention to all the same details.

The selection of proper stations for the erection of the magnetometers, and the extent of time which can be bestowed upon each, must in a great measure depend on circumstances, which can only be appreciated after the Expedition shall have sailed. The observatory at St. Helena (the officers and instruments for which will be landed by Captain Ross,) will in all probability,—and that at the Cape (similarly circumstanced in this respect) may possibly,—be in activity by the time the ships arrive at Kerguelen's Land; which we would recommend as a very interesting station for procuring a complete and as extensive a series of corresponding observations as the necessity of a speedy arrival at Van Diemen's Land for the establishment of the fixed observatory at that point will allow; taking into consideration the possibility of obtaining during the intermediate voyage, a similar series, at some point of the coast discovered by Kemp and Biscoe. In the ulterior prosecution of the voyage, a point of especial interest for the performance of similar observations will be found in New Zealand, which, according to the sketch of the voyage laid before us by Captain Ross, will probably be visited shortly after the establishment of the Van Diemen's Land observatory. The observations there will have especial interest, since, taken in conjunction with those simultaneously making in Van Diemen's Land, they will decide the important question, how far that exact correspondence of the momentary magnetic perturbations which has been observed in Europe, obtains in so remote a region, between places separated by a distance equal to that between the most widely distant European Stations.

In the interval between quitting Van Diemen's Land and returning to it again, opportunities will no doubt occur of performing more than one other series of magnetometer observations, the locality of which may be conveniently left to the judgment of Captain Ross, bearing in mind the advantage of observing at stations as remote as possible from both Van Diemen's Land and New Zealand.

The research for the southern magnetic pole and the exploration of the antarctic seas will afford, it may be presumed, many opportunities of instituting on land hitherto unknown, or on firm ice when the vessel may be for a time blockaded, observations of this description; and in the progress of the circumnavigation, the line of coast observed or supposed to exist under the name of Graham's Land, or those of the islands of that vicinity, South Shetland, Sandwich Land, and finally on the homeward voyage the Island of Tristan d'Acunha will afford stations each of its own particular interest.

A programme will be furnished of the days selected for simultaneous observations at the fixed observatories, and of the details to be attended to in the observations themselves as above alluded to. These days will include the *terms* or stated days of the German Magnetic Association, in which, by arrangements already existing, every European magnetic observatory is sure to be in full activity. These latter days, which occur four times in the year, will be especially interesting, as periods of magnetometrical observations by the Expedition, when the circumstances of the voyage will permit. For the determination of the existence and progress of the diurnal oscillation, in so far as that important element can be ascertained in periods of brief duration, it will be necessary to continue the observations hourly during the twenty-four for not less than one complete week. At every station where the magnetometers are observed, the absolute values of the dip, horizontal direction, and intensity will require to be ascertained.

Sydney, for a station of absolute determinations, would be with great propriety selected, as there can be no doubt of its becoming at no distant period a centre of reference for every species of local determination.

The meteorological particulars to be chiefly attended to, as a part of the magnetic observations, are those of the barometer, thermometer, wind, and especially auroras, if any. In case of the occurrence of the latter indeed, the hourly should at once be exchanged for uninterrupted observation, should that not be actually in operation. The affections of the magnetometers during thunder-storms, if any, should be noticed, though it is at present believed that they have no influence,

During an earthquake in Siberia in 1829, the direction of the horizontal needle, carefully watched by M. Erman, was uninfluenced; should a similar opportunity occur, and circumstances permit, it should not be neglected.

Should land or secure ice be found in the neighbourhood of the magnetic pole, every attention will of course be paid to the procuring a complete and extensive series of magnetometric observations, which in such a locality would form one of the most remarkable results of the Expedition.

ELECTROMETERS.

The Council are fully impressed with the high importance, of regular observations on the electrical state of the atmosphere; but they are not prepared to suggest any means of effecting this desirable object, which will at all correspond with the present advanced state of electrical physics. At no distant period they hope to supply a defect which is certainly a reproach to science. In the meantime much valuable information might be acquired by observations of an electroscope, on one of the ordinary constructions connected with a lofty insulated wire.

In erecting such a wire, proper precautions should be taken against accidents by preparing a sufficient conductor in its immediate vicinity, by which a communication could be at once opened with the ground in case of any sudden and dangerous accumulation of the electric fluid.

As a temporary contrivance, a common jointed fishing-rod, having a glass stick well varnished with shell lac, substituted for its smallest joint, may be projected into the atmosphere. To the end of the glass must be fixed a metallic wire terminating in a point, and connected with an electroscope by means of a fine copper wire. If the wire be made to terminate in a spiral wrapped round a piece of cotton dipped in spirits of wine and inflamed, its power of collecting electricity will be sometimes doubled, but great precautions are necessary when this mode is employed. When the electroscope has been charged, the nature of the electricity may be tested in the usual way by excited glass or sealing wax.

The principal electroscopes which are capable of being employed to ascertain the electrical state of the atmosphere, or rather to compare its state at any given elevation with the state of the medium in contact with the instrument, are the following:

1. De Saussure's electrometer, which consists of two fine wires, each terminated by a small pith ball, and adapted to a

small metal rod fixed in the upper part of a square glass cover, upon one of the faces of which a divided scale is marked, in order to measure the angles of deviation of the two balls :

2. Volta's electrometer, formed of two straws about two inches long and $\frac{1}{4}$ th of a line broad, suspended from two small very moveable rings adapted to metal rod : to measure the deviation of the straws a telescope with a nonius is employed :

3. Singer's electrometer, consisting of two slips of gold leaf suspended from the rod :

4. Bohnenberger's electroscope, formed of a single strip of gold leaf suspended from the conducting rod between two dry piles, the negative pole of one and the positive pole of the other being uppermost ; this arrangement has the advantage of indicating the kind of electricity communicated to the conductor.

The observations made with these and similar instruments have demonstrated that in serene weather the electricity of the atmosphere is always positive with regard to that of the earth, and that it becomes more and more positive in proportion to its elevation above the earth's surface ; so that if an observer be on a mountain or in a balloon, if his conductor be directed downwards to reach an inferior stratum of air, his electroscope will indicate negative electricity ; and if it be sent upwards into a superior stratum, positive electricity will be manifested. Various means have been resorted to in these experiments, such as connecting one of the extremities of the conducting wire to a kite, a small balloon, or the head of an arrow, the other extremity remaining attached to the electroscope.

It has been ascertained by the observations of De Saussure, Schubler, Arago and others, that the positive electricity of the atmosphere is subject to diurnal variations of intensity, there being two maxima and two minima during the twenty-four hours. The first minimum takes place a little before the rising of the sun ; as it rises, the intensity, at first gradually and then rapidly, increases, and arrives at its first maximum a few hours after. This excess diminishes at first rapidly and afterwards slowly, and arrives at its minimum some hours before sunset ; it re-ascends when the sun approaches the horizon, and attains its second maximum a few hours after, then diminishes till sunrise, and proceeds in the order already indicated. The intensity of the free electricity of the atmosphere has also been found to undergo annual changes, increasing from the month of July to the month of November inclusive, so that the greatest intensity occurs in winter, and the least in summer.

In cloudy weather the free electricity of the atmosphere is still positive. During storms, or when it rains or snows, the electricity is sometimes positive and sometimes negative, and its intensity is always much more considerable than in serene weather. The electroscope will, during the continuance of a storm, frequently indicate several changes, from positive to negative.

The above is a short summary of almost all that is known respecting the laws of atmospheric electricity. It will be highly important to obtain a series of observations equal in accuracy to those made by Schubler at Frankfort in 1811 and 1812, simultaneously with the observations of the hygrometer, barometer, thermometer, &c. Combined observations at a number of different stations cannot fail to give us important information respecting the distribution of the free electricity in the atmosphere, and the extent and nature of the disturbances to which it is subject; but to render the results valuable it will be necessary to have instruments comparable with each other, and this may be a difficult matter to effect.*

Very recently a new method of investigating the electric state of the atmosphere has been proposed, likely to lead hereafter to very certain and valuable results; but it has not been sufficiently put in practice to enable the Council to recommend, at the present moment, the best form of instrument for making simultaneous and comparable observations, or the proper precautions to guide the observer in manipulating it.

For the principle of this instrument we are indebted to Mr. Colladon of Geneva. He found, that if the two ends of the wire of a galvanic multiplier, consisting of very numerous coils well insulated from each other, were brought in contact, one with a body positively, and the other with a body negatively charged, a current of electricity passes through the wire, until equilibrium is restored; the energy and direction of this current is indicated by the deviation of the needle from the zero-point of the scale. This instrument is applied to the purpose of ascertaining and measuring the atmospheric electricity, by communicating one end of the wire with the earth, and allowing the other to extend into the region of the atmosphere, the electrical state of which is intended to be compared.

* For a fuller account of what is known respecting atmospheric electricity, and the mode of conducting the observations, see Becquerel's *Traité de l'Electricité*, t. iv. pp. 78—125.

Thunder storms, of course, should be attended to ; but it is of consequence also to notice distant lightning not accompanied with thunder audible at the place of observation, especially if it take place many days in succession, and to note the quarter of the horizon where it appears, and the extent which it embraces. In an actual thunder storm, especial notice should be taken of the quantity of rain which falls, and of the fits or intermittances of its fall, as corresponding, or not, to great bursts of lightning, as also of the direction of the wind, and the apparent progress of the storm with or against it.*

REGISTERS.

The Register proposed by the Council may be comprised in two skeleton forms, which have been supplied to the magnetic observatories and to the Expedition.

They are each calculated for one month's observation. *The first form* is for the insertion of observations as they are made in their uncorrected state. It consists of 12 principal divisions, and is ruled across for 31 days, and for the arithmetical convenience of casting up the sums and means of the quantities inserted. At the bottom of the sheet there is also a space provided for the hourly observations of the barometer and thermometers on *the twenty-first day of the month*, which will be more particularly described after the explanation of the principal divisions.

The outside compartments, both on the left and right of the sheet, are for the date of the month and the phases of the moon.

The second compartment is for the height of the barometer, and the temperature of the mercury for the four regular periods of observation.

The third compartment is appropriated to the dew-point hygrometer, and contains also four columns for the four daily observations, each of which is subdivided into three ; for the temperature of the air, the dew-point, and the difference between the two.

The fourth compartment is for the wet-bulb hygrometer, and is similarly divided and subdivided for the temperature of the dry- and wet-bulb thermometer, and for their differences.

The fifth compartment is prepared for the maxima and

* On these subjects the Council especially recommend the attentive perusal of Arago's *Notice sur le Tonnerre*.

minima of temperature, and is divided into three. In the first division are to be recorded the maxima and minima of thermometers carefully placed in the shade and screened from radiation. In the second, the maxima of a blackened thermometer exposed to the sun, and the minima of a thermometer placed in a metallic mirror, and radiating freely to the clear sky. The third is devoted to occasional observations of the actinometer under favourable circumstances.

The sixth compartment is for the temperature of the surface-water of the sea, or of any river in the immediate neighbourhood of the observatory.

The seventh compartment is prepared for observations upon the direction and force of the wind at the four regular hours of registry. In the left-hand column of each division is to be recorded the direction of the vane, and in the right-hand column the height of Lind's gauge, in tenths of an inch of water.

In the eighth compartment the amount of rain is to be registered once in the day; and in the ninth, the electrical state of the atmosphere, if possible, at the four periods, 3 A. M., 9 A. M., 3 P. M., and 9 P. M.

The tenth compartment is appropriated to remarks on the clouds, and weather generally; and in the eleventh is to be noted, at noon, the longitude and latitude at sea.

On a careful review of the month's observations, the maxima and minima results should have the algebraic signs + and — respectively affixed.

The second form is devoted to the corrected results of the observations, and to the optical comparison together of some of them, by their projection upon a scale of equal parts.

The upper half of the sheet is vertically divided into two equal parts, each prepared for half the month's observations, and accordingly ruled across into sixteen spaces for the daily observations, and two for the sums and means of the quantities. Each half is also divided into five compartments.

The first is for the date of the month and the phases of the moon.

The second for the corrected height of the barometer at 32° Fahr.

The third is appropriated to the elastic force of the aqueous vapour corresponding to the dew-point, and which may be taken from Table 5, in the Appendix B.

The fourth is for the maximum and minimum of temperature, and the mean of the two.

And the fifth for occasional remarks.

The lower half of the sheet is also vertically divided into

two equal parts, each of which is similarly divided into 31 columns for the daily observations of a month; and these again subdivided into four, for the six-hourly observations of each day. The vertical lines thus formed are divided into 6 inches; and each inch into tenths of an inch, and half-tenths, by horizontal lines.

The left-hand compartment thus ruled, is intended for the projection of curves of temperature; for this purpose each tenth of an inch upon the scale must be reckoned a degree, which will be divided by the faint line into halves.

The value of the degree may be arbitrarily fixed, and inserted in the margin according to convenience. Towards the upper part of the scale the results of the six-hourly observations should each be marked by a dot in its appropriate space, and the dots may be afterwards connected by a line.

The temperatures of the dew-point, or of the wet-bulb thermometer, or the mean temperature, may be compared with this primary result by projecting their curves in a similar way beneath it; and should the observations of these points be less frequent than four times in the day, the daily spaces may easily be divided accordingly.

The right-hand compartment is appropriated to the projection of curves of pressure, and the four daily observations of the barometer are to be marked by dots towards the upper part of the scale of inches, and afterwards connected by a line. Towards the lower part of the scale the elastic force of the vapour is to be noted, and the marks to be similarly connected by a line.

On either the scale of temperature or of pressure, occasional comparisons may be made with results obtained at other stations, which, if judiciously selected, cannot fail to prove of high interest and importance. They should, however, be laid down in pencil, or marked by a fainter line.

At the bottom of the first skeleton form will be found a space prepared for the 24 hourly observations of the *twenty-first day* of the month, both in their uncorrected and their corrected state. It is divided into four compartments for 6 hours each. The instruments which can with most facility be observed in this manner, are the barometer with its attached thermometer, and the dry-and wet-bulb thermometers; and columns are appropriated to each of these. It is desirable that the mean of each 6 hour should be calculated, and spaces have been provided accordingly for the arithmetical operations.

In casting up the sums and calculating the *means*, care should be taken in all cases to verify the results by repetition;

and the Council recommend in every instance, before adding up the columns, to look down each to see that no obvious error of entry (as of an inch in the barometer, a very common error) may remain to vitiate the mean result. The precaution should also be taken of counting the days in each column, so as to make no mistake in the divisor.

The skeleton forms will be interleaved with blank pages, to facilitate computations and comparisons, and to afford space for other observations of atmospheric phenomena, which will perpetually present themselves to those who make it their business or their pleasure to watch the changes of the weather on a judicious plan. The Council, indeed, wish it to be understood, that, in the suggestions which they have offered, they have taken into consideration only such observations as are indispensable for laying the first foundations of meteorological science; some investigations of a more refined character they may, probably, make the subject of a future report.

As soon as the register of a month's observations has been computed, it should be copied, and the copy carefully compared with the original by two persons, one reading aloud from the original, and the other attending to the copy, and then exchanging parts,—a process always advisable whenever great masses of figures are required to be correctly copied.

A copy so verified should be transmitted regularly to such person or public body, as, under the circumstances, may be authorized or best adapted to receive and discuss the observations.

ACCOUNT OF THE MAGNETICAL INSTRUMENTS EMPLOYED AND OF THE MODE OF OBSERVATION TO BE ADOPTED, IN THE MAGNETICAL OBSERVATORIES ABOUT TO BE ESTABLISHED BY HER MAJESTY'S GOVERNMENT.

THE elements on which the determination of the earth's magnetic force is usually based are, the *declination*, the *inclination*, and the *intensity*. If a vertical plane be conceived to pass through the direction of the force, that direction will be determined when its inclination to the horizon is given, as well as the angle which the plane itself forms with the meridian; and if, in addition to these quantities, we likewise know the number which expresses the ratio of the intensity of the force to some established unit, it is manifest that the force is completely determined.

For many purposes, however, and especially in the delicate researches connected with the *variations* of the magnetic force, a different system of elements is preferable. The intensity being resolved into two portions in the plane of the magnetic meridian, one of them *horizontal* and the other *vertical*, it is manifest that these two components may be substituted for the total intensity and the inclination; while, at the same time, their changes may be determined with far greater precision. The former variables are connected with the latter by the relations

$$X = R \cos \theta, \quad Y = R \sin \theta;$$

in which R denotes the intensity, X and Y its horizontal and vertical components, and θ the inclination; and the variations of θ and R are expressed in terms of the variations of X and Y by the formulæ:

$$d\theta = \frac{1}{2} \sin 2\theta \left(\frac{dY}{Y} - \frac{dX}{X} \right);$$

$$\frac{dR}{R} = \cos^2 \theta \frac{dX}{X} + \sin^2 \theta \frac{dY}{Y}.$$

As the instruments destined for the observation of these elements (with a set of which each observatory is furnished) are, for the most part, novel in form, it will be useful to give a somewhat detailed account of their construction and various adjustments, before entering on the plan of observation to be pursued.

DECLINATION MAGNETOMETER.

Construction.—The essential part of the declination magnetometer is a magnet bar, suspended by fibres of untwisted silk, and inclosed in a box, to protect it from the agitation of the air. The bar is a rectangular parallelepiped, 15 inches in length, $\frac{7}{8}$ ths of an inch in breadth, and $\frac{1}{4}$ th of an inch in thickness. In addition to the stirrup by which the bar is suspended it is furnished with two sliding pieces, one near each end. One of these pieces contains an achromatic lens, and the other a finely divided scale of glass; the scale being adjusted to the focus of the lens, it is manifest that the apparatus forms a moving collimator, and that its absolute position at any instant, as well as its changes of position from one instant to another, may be read off by a telescope at a distance. The aperture of the lens of this collimator is $1\frac{1}{4}$ inch, and its

focal length about 12 inches. Each division of the scale is $\frac{1}{15}$ th part of an inch; and the corresponding angular quantity is about 43 seconds.

To the suspension thread is attached a small cylindrical bar, the ends of which are of smaller diameter, and support the stirrup which carries the magnet. The apertures in the stirrup, by which it hangs on the cylinder, are of the form of inverted Y's, so that the bearing points are invariable. A second pair of apertures at the other side of the magnet, serves for the purpose of *inversal*; and care has been taken to render the lines connecting the bearing points of each pair of Y's parallel, so that there may be no difference in the amount of torsion of the thread in the two positions of the stirrup. The two pairs of apertures are at different distances from the magnet, in order that the line of collimation may remain nearly at the same height on *inversal*, and thus it may not be necessary to alter the length of the suspension thread. The stirrup, and the other sliding pieces, are formed of gun metal.

For the purpose of taking out the torsion of the suspension thread, the apparatus is furnished with a *detorsion bar*, which (with its appendages) is of the same weight as the magnet. It is a rectangular bar of gun-metal, furnished with a stirrup and collimator similar to those of the magnet. A rectangular aperture in the middle receives a small magnet, the use of which is to impart a slight directive force to the suspended bar, and without which the final adjustment of detorsion would be tedious and difficult.

The frame-work of the instrument consists of two pillars of copper, 35 inches in height, firmly screwed to a massive marble base. These pillars are connected by two cross pieces of wood, one at the top, and the other 7 inches from the bottom. In the centre of the top piece is the suspension apparatus, and a divided circle used in determining the amount of torsion of the thread. A glass tube (between this and the middle of the lower cross piece) encloses the suspension thread; and a glass cap at top covers the suspension apparatus, and completes the enclosure of the instrument.

The box is cylindrical, its dimensions being 20 inches in diameter by 7 inches in depth. It rests upon the marble slab, and encompasses the pillars; and it is so contrived as to be raised, when necessary, for the purpose of manipulation. There are two apertures in the box, opposite to each other. The aperture in front, used for reading, is covered with a circular piece of parallel glass, attached to a rectangular frame of wood which moves in dovetails; the prismatic error of the glass (if any) is corrected by simply reversing the

slider in the dovetails. The opposite aperture is for the illumination of the scale.

In addition to the parts abovementioned, the instrument is provided with a second magnet, of the same dimensions as the first, to be used in measurements of absolute intensity; a thermometer, the bulb of which enters the box, in order to determine the interior temperature; and a copper ring, for the purpose of checking the vibrations.

Adjustment.—The instrument having been placed on its support, the base is to be levelled, and the whole then fixed in its place. The levelling of the base may conveniently be performed by the aid of a plumb-line hanging in the place of the suspension thread; but no great precision is required in this operation, the chief object of which is that the suspension thread may occupy the middle of the tube, and that the magnet may be central with regard to its support. The suspension thread is then to be formed, and attached at one extremity to the roller of the suspension apparatus, and at the other to the small cylinder which is to bear the stirrup and magnet. Sixteen fibres* of untwisted silk are sufficient to bear double the load without breaking, and will be found to form in other respects a convenient suspension.

These preparations being made, the adjustments are the following:

1. The sliders being placed on the magnet, the scale is to be adjusted to the focus of the lens, and in such a manner that the centre of gravity of the sliders may be near the middle of the bar. The adjustment to focus has been already made by the artist, and the corresponding distances of the sliders measured; they will be found in Table 1.

2. The magnet is to be connected with the suspension thread by means of the stirrup, and to be moved in the stirrup until it assumes the horizontal position. This adjustment may be conveniently effected by means of the image of the magnet, reflected from the surface of water or mercury, the object and its reflected image being parallel when the former is horizontal. The stirrup is then fastened by its screws, and the magnet wound up to the desired height. As the thread stretches considerably at first, allowance should be made for this in the height.

- 3.† The magnet is then removed, and the unmagnetic bar

* Not the individual fibre of the silk-worm, but the compound fibre in the state in which it is prepared for spinning.

† It is obvious that this step of the adjustment may precede the 1st and 2nd, where a saving of time is important.

(having its collimator similarly adjusted) is to be attached, without its small magnet, and allowed to swing for several hours. The bar having come to rest, or nearly so, its deviation from the magnetic meridian is to be *estimated*, and the moveable arm of the torsion circle turned through the same angle in an opposite direction. The plane of detorsion then coincides, approximately, with the magnetic meridian.

4. The magnet is then to be substituted for the unmagnetic bar, and the telescope being directed towards the collimator, the point of the scale coinciding with the vertical wire is to be noted when the magnet is in the *direct* and *inverted* positions. Half the sum of these readings is the point of the scale corresponding to the magnetic axis of the magnet bar; and half their difference (converted into angular measure) is the deviation of the line of collimation of the telescope from the magnetic meridian. The telescope should be moved through this angle in the opposite direction.

5. In order to take out the remaining torsion of the thread, the magnet is again to be removed, and the unmagnetic bar (with its small magnet attached) substituted. The deviation of this bar from the magnetic meridian should then be read off on its divided scale, and the moveable arm of the torsion circle turned through a given angle in the opposite direction. The deviation being again read, a simple proportion will give the remaining angle of torsion; and the moveable arm being turned through this angle in the opposite direction, another observation will serve to verify the adjustment. The plane of detorsion then coincides with the magnetic meridian; and the magnet being replaced, the instrument is ready for use.

Observations.—The observations to be made with this instrument are, 1. of the *absolute declination*; 2. of the *variations of the declination*; and 3. of the *absolute intensity*.

For measurements of the *absolute declination* each observatory is furnished with a small transit instrument having an azimuth circle. This instrument being placed in the magnetic meridian of the declination instrument, the point of the scale coinciding with the central wire of the transit telescope is to be observed; the interval between this point and the point*

* In determining this point by the mean of two readings of the scale with the bar erect and inverted, care must be taken to eliminate the declination changes which may occur in the interval of the two parts of the observation. The horizontal force magnetometer may be applied to the purpose of this elimination. But perhaps the simplest course is to take a *series* of readings as rapidly as possible, alternately in the two positions of the bar, choosing

corresponding to the magnetic axis of the bar, converted into angular measure, is the deviation (δ) of the line of collimation of the transit telescope from the magnetic meridian. The verniers of the horizontal circle being then read, the telescope is turned, and its central wire made to bisect a distant mark, whose azimuth (α) has been accurately determined. If α denote the angle read off on the horizontal circle, it is manifest that the angle between the magnetic and the astronomical meridians is

$$\alpha + \alpha + \delta,$$

α and δ being affected with their proper signs. The angle α is supposed to have been previously determined by the help of the transit instrument.

But instead of referring the transit telescope *directly* to the magnetic meridian by means of the moving collimator, the same result will be obtained, and probably in a better manner, by referring it to the line of collimation of the *fixed telescope*, with which the changes of the declination are regularly observed. For this purpose it is only necessary to employ the latter telescope as a collimator, the telescope being *reversed* in its Y supports, if necessary. A fixed collimator may also be conveniently substituted for the distant mark. This mode of observation has the advantage of connecting the absolute determination directly with the regular series of observations; and it is manifest that it is sufficient, without any other means, to determine whether any, and what changes may have occurred in the position of the fixed telescope.

The fixed telescopes, furnished to each observatory, have an aperture of $1\frac{1}{2}$ inches, and focal length of 14 inches. They should be fixed upon a stone pillar, or upon a firm pedestal of wood resting on solid masonry unconnected with the floor.

In observing the *declination changes* the fixed telescope (above referred to) is alone employed. The observation consists simply in noting the point of the scale coinciding with the vertical wire, at three successive limits of the arc of vibration. The three readings being denoted by a , b , c , the mean point of the scale corresponding to the time of the middle observation is

$$\frac{1}{2} (a + 2 b + c).$$

for the time of observation a period when the declination changes are slow and regular. By comparing each result with the mean of the preceding and subsequent, and then taking the mean of all these partial means, a very accurate determination may be obtained.

This mode of observation is sufficient where the observer is not limited to a *precise moment* of observation. Otherwise the more exact method pointed out by Gauss is to be preferred.

The changes of position of the scale may be converted into angular measure, the angle corresponding to one division, being known. In general, however, this reduction will only be required in the monthly mean results.

Before the true changes of the declination can be deduced from the observed readings, it is necessary to apply a correction depending upon the force of torsion of the suspension thread. For supposing that the plane of detorsion has been brought (by the adjustments above described) to coincide with the magnetic meridian, it is manifest that on every deviation of the magnet from, that, its mean position, the torsion force will be brought into play; and as this force tends to bring back the magnet to the mean position, the apparent deviations must be less than the true. The ratio of the torsion force, to the magnetic directive force, is experimentally determined by turning the moveable arm of the torsion circle through any given large angle (for example 90°), and observing the corresponding angle through which the magnet is deflected. Let u denote the latter angle, and v the former; then the ratio in question is,

$$\frac{G}{F} = \frac{u}{v-u};$$

in which G is the co-efficient of the torsion force, and F the moment arising from the action of the earth's magnetic force upon the free magnetism of the bar, the direction of the action being supposed to be perpendicular to its magnetic axis. The ratio of the two forces being thus found, the true declination changes are deduced from the apparent, by multiplying them by the co-efficient

$$1 + \frac{G}{F}.$$

In order to obtain an exact result by the mode of experiment above described, it is necessary that the *actual* changes of the declination which may occur in the interval of the two readings, should be eliminated. The obvious method of accomplishing this, is to observe the declination changes

simultaneously with a second apparatus. If such means, however, should not be at hand, the object may be attained by making a *series* of readings with the vernier of the torsion circle alternately in two fixed positions (for example $+90^\circ$ and -90°); the mean result will be independent of the declination changes, provided the progress of these changes has been gradual in the interval of the experiment.

For the purpose of determining the *absolute intensity* of the horizontal component of the earth's magnetic force, the declination instrument is provided with a *deflecting bar*, and a *beam compass* to be used in measuring its distance from the suspended magnet. The mode of observation has been so fully explained by Gauss, in his valuable memoir entitled "*Intensitas vis terrestris ad mensuram absolutam revocata*," and in the first volume of the "*Resultate*," that it is unnecessary to enter here into any details.

The following table contains the interval of the sliders of the collimators, corresponding to focal adjustment; and also the arc values of one division of the scale in each instrument, expressed in decimals of a minute.

TABLE 1.

No. of Instrument.	Observatory.	Interval of Sliders.	Arc value of one division.
		inches.	
I.	H. M. S. Erebus.....	11.70	0.7267
II.	Van Diemen's Land	12.01	0.7085
III.	Montreal.....	11.72	0.7208
IV.	Cape of Good Hope	11.18	0.7525
V.	St. Helena.	11.96	0.7108

HORIZONTAL FORCE MAGNETOMETER.

The instrument employed in determining the horizontal component of the earth's magnetic force is similar, in principle, to the "*bifilar magnetometer*" of Gauss. It is a magnet bar, suspended by two equi-distant wires, or (more accurately) by two portions of the same wire, the distance of whose bearing points is the same above and below; by the rotation of the upper extremities of the wire round their middle point, the magnet is maintained in a position at right angles to the magnetic meridian.

It is manifest from the nature of this suspension, that the *weight* of the suspended body will tend to bring it into the position in which the two portions of the wire are in the *same plane* throughout. The moment of the directive force is $G \sin v$;— v denoting the angle formed by the lines joining the bearing points above and below, or the deviation from the plane of detorsion; and G being expressed by the formula

$$G = w \frac{a^2}{l};$$

in which w denotes the weight of the suspended body, a half the interval of the wires, and l their length. The earth's *magnetic force*, on the other hand, tends to bring the magnetic axis of the bar into the magnetic meridian with the force $F \sin u$; in which u is the deviation of the magnetic axis from the meridian, and F is the product of the horizontal part the earth's magnetic force into the moment of free magnetism of the bar. The magnet being thus acted on by two forces, will rest in the position in which their moments are equal. When the instrument is so adjusted that $u = 90^\circ$, or the magnet at right angles to the magnetic meridian,

$$F = G \sin v;$$

and the ratio of the forces is known, when we know the angle v . But as one of these forces is constant, and the other variable, it is evident that the place of the magnet will vary around its mean position, and that the variations of angle are connected with the variations of the force. This connexion is expressed by the formula

$$dF = F \cotan v \cdot du;$$

the angle du being expressed in parts of radius.

Construction.—The magnet bar is of the same dimensions as that of the declination instrument. The collimator, by which its changes of position are observed, is attached to the stirrup, and has a motion in azimuth. The suspending wire passes round a small grooved wheel, on the axis of which the stirrup rests by inverted Y's; and the instrument is furnished with a series of such wheels, whose diameters increase in arithmetical progression, (the common difference being about $\frac{1}{160}$ th of an inch,) for the purpose of varying the interval of the wires. The exact intervals, corresponding to each separate wheel, have been determined by the artist by accurate micrometrical measurements; they are given in Table III. The same interval is altered, at the upper extremity, by means of two screws (one right-handed and the other left-

handed) cut in the same cylinder; the wires being lodged in the intervals of the threads, and their distance regulated by a micrometer head. The interval of the threads of this screw (which is precisely the same for all the instruments) is $\frac{1}{100}$ ths, or .02597 of an inch. The micrometer head is divided into 100 parts; and, as one revolution of the head corresponds to *two* threads of the screw, a single division is equivalent to .0005194, or the $\frac{1}{1940}$ th of an inch nearly. The micrometer head has been carefully adjusted by the artist, so that the index is at zero, when the interval of the wires is exactly half an inch.

The collimator, in this instrument, is enclosed in a light tube attached to the stirrup. The aperture of the lens is about $\frac{1}{10}$ ths of an inch, and its focal length about 8 inches. The divisions of the scale are the same as in the collimator of the declination magnetometer; the corresponding arc values have been ascertained for each instrument by accurate experiment, and are given in Table II.

The larger parts of this apparatus,—the box, the framework, and the support,—are precisely similar to those of the declination magnetometer. In addition to the parts already described, the instrument is furnished with a spare magnet; a brass weight, required in determining the plane of detorsion of the wires relatively to the magnetic meridian; a thermometer, the bulb of which is within the box, for the purpose of ascertaining the interior temperature; and a copper ring used in checking the vibrations.

Adjustments.—The instrument being placed on its support, the base is to be levelled, and the whole apparatus fixed. Having then selected one of the small grooved wheels, and fixed it, temporarily, with its axis horizontal, the wire is to be passed round it; and the free extremities of the wire being passed through the corresponding holes in the suspension roller, placed beneath, weights are to be attached, and the two portions of the wire allowed to assume their natural position; the extremities may then be *fastened* to the roller, by introducing small wooden plugs in the holes. The parts are then to be inverted, and put in their proper places; the suspension apparatus resting on the divided circle, and the wire hanging down the tube.

The collimator (its scale having been previously adjusted to focus*) is to be screwed on to the stirrup, and the latter attached to the axis of the grooved wheel by means of its Y's. The magnet is then introduced into the stirrup and

* This adjustment has been already made by the artist.

levelled; and the wires wound upon the roller, until the collimator is at the desired height.

These preparations being made, the adjustments are the following :

1, Determine experimentally the angle through which it is necessary to turn the moveable arm of the torsion circle, in order to deflect the magnet from the magnetic meridian to a position at right angles to it, the two positions being merely *estimated*. The cosine of this angle is, approximately, the ratio of the magnetic force to the torsion force, or the value

of the fraction $\frac{F}{G}$. The nearer this ratio is to unity, the

more delicate will be the instrument; practically, $\frac{F}{G}$ will be found a convenient value. If, on making the foregoing experiment, the ratio should be found to fall below, or to exceed the proper limits, the torsion force must be altered by introducing a different wheel, and making the corresponding alteration in the interval of the upper extremities of the wires.

2. The magnetic axis being brought, approximately, into the magnetic meridian, by turning the moveable arm of the torsion circle, the collimator is to be turned, by its independent motion, until some point about the middle of the scale coincides with the vertical wire of the fixed telescope. This point of the scale is to be noted in the usual manner.

3. The magnet is then to be removed, and the brass weight attached. Note the new point of the scale which coincides with the wire of the telescope. Then, if the magnet had been placed (in the previous experiment) in its *direct* position (i. e. north to north) the error of the plane of detorsion is

$$v \left(\frac{G}{F} + 1 \right).$$

v being the difference of the two readings, converted into angular measure. If, on the other hand, the magnet had been *reversed* (i. e. north end to south) the error is

$$v \left(\frac{G}{F} - 1 \right).$$

The moveable arm of the torsion circle is then to be turned through this angle, in the opposite direction; and the magnetic axis will be in the magnetic meridian.

The difference of the two readings, corresponding to a given error, being much greater in the reversed than in the

direct position of the magnet, it follows that the former affords a much more delicate method of making the desired adjustment.

4. The brass weight remaining attached, turn the moveable arm of the torsion circle through 90° . Then turn back the collimator, until some point about the middle of the scale coincides with the verticle wire of the fixed telescope; and note the reading.

5. Now remove the brass weight, and replace the magnet. The magnetic force of the earth will bring it back towards the magnetic meridian, and the scale will be thrown out of the field of the telescope. Then turn the moveable arm of the torsion circle until the point of the scale last noted is brought to coincide again with the wire of the telescope; the magnetic axis is then in the plane perpendicular to the magnetic meridian, and the adjustment is complete.

Observations.—The observations to be made with this instrument are those of the *absolute* value of the *horizontal intensity*, and its *changes*.

From the explanation of the principle of the instrument, given above, it is manifest that it will serve to determine the moment of the force exerted by the earth upon the free magnetism of the suspended bar. Let X denote (as before) the horizontal part of the earth's magnetic force; m the moment of free magnetism of the bar; then

$$m X = F,$$

F having the same meaning as before (page 230.) Hence, substituting the values of F and G , we have

$$m X = w \frac{a^3}{l} \sin v;$$

in which equation all the quantities of the second member may be obtained by direct measurement. The chief difficulty in this method consists in the determination of the quantity a , which should be known to a very small fractional part of its actual value. This difficulty has been overcome by the measuring apparatus connected with the suspension, which (as has been already stated) serves to determine the interval of the wires, at their upper extremity, to the $\frac{1}{1000}$ th of an inch. The numbers given in Table III. for the lower interval, may be relied on to the same degree of accuracy. It is scarcely necessary to mention that the length of the wires, l , is to be measured between the points of contact above and below.

The *product* of the earth's magnetic force into the magnetic

moment of the bar being thus known, the *ratio* of the same quantities is to be determined by removing the bar from the stirrup, and using it to *deflect* the suspended bar of the declination instrument, according to the known method devised by Gauss. The experiments of deflection may, however, be performed without the aid of the second magnetometer, by operating upon another bar placed in the *reverse* position. This method has even the advantage in point of delicacy; but it labours under the disadvantage of requiring that the value of $\frac{F}{G}$ should be determined for the second bar.

The chief use of this apparatus is in observing the *variations* of the intensity. In these observations it is only necessary to note, at any moment, the point of the scale coinciding with the vertical wire of the fixed telescope, the mode of observing being precisely the same as in the other instrument. Let n be the number of divisions, and parts of a division, by which the reading at any moment differs from its mean value; then the corresponding variation of the angle (in parts of radius) is

$$du = na;$$

a denoting the arc value (in parts of radius) corresponding to a single division. Substituting this in the formula of page 459, we have

$$\frac{dF}{F} = na \cotan v = kn;$$

k being the value of the constant co-efficient $a \cotan v$. The values of a have been determined for each of the instruments, and are given in Table II.

The quantity F , in the preceding formula, is the product of the earth's magnetic force into the moment of free magnetism of the bar; and as the latter quantity varies with the temperature, it is necessary to apply a correction, before we can infer the true changes of the earth's force. This correction is easily deduced. Since $F = Xm$, there is

$$\frac{dF}{F} = \frac{dX}{X} + \frac{dm}{m};$$

so that the correction to be applied, in order to deduce the value of $\frac{dX}{X}$, is $-\frac{dm}{m}$. Let t denote the temperature, in

degrees of Fahrenheit; q the relative change of the magnetic moment corresponding to one degree; then

$$\frac{d m}{m} = q (t - 32).$$

Accordingly, the changes of the earth's force will be expressed by the formula

$$\frac{d X}{X} = k n + q (t - 32).$$

It is not necessary that these reductions should be applied to the individual results, except in cases of marked change, where it is desired to trace the progress of the actual phenomena. The results should be recorded as they are observed, in parts of the scale; and the reductions made in the monthly, or other mean values.

Table II. contains the arc values of one division of the scale, in each instrument, expressed in *decimals of a minute*; as also the same quantities reduced to *radius*, as the unit by multiplying by the number '0002909.

Table III. contains the intervals of the axis of the wires corresponding to each wheel, in decimals of an inch; the wire used being that designated in commerce as "silver fine 6."

TABLE II.

No. of Instrument.	Observatory.	Arc values of one division.	
		In Minutes.	In parts of Radius.
I.	H.M.S. Erebus	1'075	'0003127
II.	VanDiemen's Land	1'080	'0003142
III.	Montreal.....	1'074	'0003124
IV.	Cape of Good Hope	1'084	'0003153
V.	St. Helena	1'080	'0003142

TABLE III.

No. of Wheel.	I. H.M.S. Erebus.	II. Van Diemen's Land.	III. Montreal	IV. Cape of Good Hope.	V. St. Helena.
1	·2536	·2549	·2529	·2542	·2536
2	·3032	·3058	·3055	·3055	·3065
3	·3529	·3516	·3529	·3497	·3513
4	·4058	·4088	·4078	·4052	·4071
5	·4562	·4555	·4581	·4555	·4545
6	·5055	·5071	·5042	·5055	·5058
7	·5555	·5604	·5588	·5565	·5591
8	·6071	·6071	·6071	·6097	·6081

VERTICAL FORCE MAGNETOMETER.

The instrument used in determining the changes of the *vertical component* of the magnetic force is a magnetic needle resting on agate planes, by knife edges, and brought to the horizontal position by weights. From the changes of position of such a needle, the changes of the vertical force may be inferred, when we know the mean inclination at the place of observation, the azimuth of the plane in which the needle moves, and the angle which the line connecting the centre of gravity and centre of motion makes with the magnetic axis. As, however, the determination of this latter constant would involve the necessity of considerable additions to the apparatus, the plan adopted has been to *adjust* the needle so that the angle in question shall be *nothing*. The centre of gravity being thus brought to some point of the magnetic axis, the changes of the vertical force are connected with the changes of the position of the needle by the formula.

$$\frac{\delta F}{F} = \cos \alpha \cdot \cotan \cdot \theta \, d\zeta;$$

$d\zeta$ denoting the change of angle in parts of radius, α the *azimuth* of the plane in which the needle moves, and θ the *inclination*.

Construction.—The magnetic needle is 12 inches in length. It has a cross of wires at each extremity, attached by means of a small ring of copper; the interval of the crosses being 13 inches. The axis of the needle is formed at one part into a *knife edge*, and at the opposite into a portion of a *cylinder*,

having this edge for its axis, the edge being adjusted to pass as nearly as possible through the centre of gravity of the unloaded instrument. The weights by which the other adjustments are effected are small brass screws moving in fixed nuts, one on each arm; the axis of one of the screws being *parallel* to the magnetic axis of the needle, and that of the other *perpendicular* to it.

The agate planes upon which the needle rests are attached to a solid support of copper, which is firmly fixed to a massive marble base. In this support there is a provision for raising the needle off the planes, the contrivance for effecting this object being similar to that employed in the inclination instrument. The whole is covered with an oblong box of mahogany, in one side of which are two small glazed apertures, for the purpose of reading; the opposite side of the box is covered with plate glass. A thermometer, within the box, shows the temperature of the interior air; and a spirit level, attached to the marble base, serves to indicate any change of level which may occur in the instrument.

The position of the needle at any instant is observed by means of two micrometer microscopes, one opposite each end. These microscopes are supported on short pillars of copper, attached to the base of the instrument. They are so adjusted that one complete revolution of the micrometer screw corresponds to 5 minutes of arc. The micrometer head is divided into 50 parts; and, consequently, the arc corresponding to a single division is $0^{\circ}.1$.

In addition to these parts, the apparatus is provided with a brass bar of the same length as the magnet, (furnished like it with cross wires at the extremities, and knife-edge bearings,) for the purpose of determining the zero points of the microscopes; a brass scale, divided to $10'$, used in adjusting the value of their divisions; and a horizontal needle, to be employed in determining the azimuth of the vertical plane in which the needle moves.

Adjustments.—The following are the adjustments required in this instrument:

1. The instrument being placed on its support, in a suitable position with respect to the other two instruments, the azimuth of the plane in which the needle is to move may be adjusted in the following manner. The plane is made to coincide, in the first instance, with the magnetic meridian, by means of the horizontal needle which moves upon a pivot fixed to the top of the scale. A small theodolite (or other instrument for measuring horizontal angles) is then placed on the base; and its telescope brought to bear on a distant mark. The teles-

cope should then be moved through a horizontal angle equal to the intended azimuth of the instrument, but in an opposite direction. The base of the instrument is next to be turned, without disturbing the theodolite, until the mark is again bisected by the wires of the telescope: it is then in the required azimuth. The base should then be levelled, and permanently fixed.

2. The microscopes should now be adjusted, 1. to bring the image of the cross wires of the needle to coincide with the wires of the microscopes; and 2. to make the arc value of the interval of the wires, corresponding to one revolution of the micrometer head, exactly equal to five minutes*. These arrangements have been nearly effected in the first construction of the instrument; for the purpose of completing the adjustment, the microscopes are capable of a double motion, one of the entire body of the instrument, and the other of the object glass alone. It is manifest that these two movements are sufficient to effect both adjustments. The former is attained when the cross of wires is seen distinctly (and without parallax) at the same time that the microscope wires are exactly in the focus of the eye-piece; the latter is accomplished when the moveable wire of the microscope is made to pass over a given number of divisions of the scale, by double the number of *complete* revolutions of the micrometer head.

3. The *fixed* wires of the microscopes are then to be adjusted to the same *horizontal* line. This is effected by means of the brass needle. This needle being placed upon the agate planes, by its knife edges, and allowed to come to rest, it is manifest that the line joining the cross wires will be horizontal, provided it be perpendicular to the line joining the centre of gravity and the axis. To effect this latter adjustment, the needle (a great part of whose weight is disposed below the knife edge) is furnished also with a small moveable weight. The test of the adjustment is similar to that of the corresponding adjustment of the ordinary balance. The moveable wire of one of the microscopes being brought to bisect the cross, if the adjustment is complete, it will bisect the cross at the other extremity upon reversal; if not, the position of the needle will indicate in what manner the weight is to be moved.

* This adjustment is by no means a necessary one. It is sufficient for all purposes if the arc value corresponding to one revolution of the micrometer be accurately known.

A horizontal line being thus obtained, the fixed wires of the microscopes are to be adjusted to it, by moving the capstan-headed screws with which they are connected.

4. The last adjustment is that of the magnetic needle itself. This adjustment is twofold: 1. of the needle to the horizontal position; and 2. of the centre of gravity of the needle to the magnetic axis. To effect this double adjustment the needle is furnished with two moving weights, one on each arm. These weights (it has been already stated) are screws moving in fixed nuts, one in a direction parallel to the magnetic axis of the needle, and the other in a direction at right angles to it. By the movement of the former the needle is brought to the horizontal position; and by that of the latter, the centre of gravity is made to coincide with the magnetic axis. The latter part of the adjustment is tested by inverting the needle on its supports; the inclination of the needle should not be altered by this inversion when the adjustment is complete.

Observations.—In observing the variations of the vertical force with this instrument, it is only necessary to bring the moveable wire of each micrometer to bisect the opposite cross of the needle; unless in seasons of disturbance, the needle will be found at each instant to have assumed its position of equilibrium. The interval between the fixed and moveable wires, expressed in angular measure, is the deviation of the needle of the horizontal position; and the changes of the vertical force are thence obtained by multiplying by a constant coefficient.

If n denote the number of minutes, and parts of a minute, in the observed angle of deviation, the changes of the force are expressed (as in the case of the other component) by the formula.

$$\frac{d F}{F} = k n;$$

in which the constant coefficient is

$$k = \cos a \cotan \theta \sin 1'.$$

The quantity F in the preceding formula is the product of the vertical component of the earth's magnetic force multiplied by the moment of free magnetism of the needle; or

$$F = m Y.$$

Accordingly the results thus deduced require a correction for the effects of temperature upon the quantity m . This correc-

tion is similar to that applied to the horizontal intensity; and the corrected expression of the changes of the vertical component is accordingly

$$\frac{dY}{Y} = k n + q (t - 32);$$

where t denotes the actual temperature (in degrees of Fahrenheit) at the time of observation, and q the relative change of the magnetic moment of the needle corresponding to one degree. As in the case of the other instruments, however, it is not in general necessary to apply these reductions to the individual results.

TIMES OF OBSERVATION.

The objects of inquiry in terrestrial magnetism may be naturally classed under two heads, according as they relate, 1. to the *absolute* values of the magnetic elements at a given epoch, or their mean values for a given period; or 2. to the *variations* which these elements undergo from one epoch to another. It will be convenient to consider separately the observations relating to these two branches of the subject.

ABSOLUTE DETERMINATIONS.

By the method of observation which has been suggested for the *absolute declination*, every determination of the position of the declination bar is rendered absolute. We have only to consider the varying angle between the magnetic axis of the bar and the line of collimation of the fixed telescope, as a correction to be applied to the constant angle (already determined) between the latter line and the meridian. It is manifest that if the *fixity* of the line of collimation of the telescope could be depended on, a single determination of the latter angle would be sufficient. But this is not to be trusted for any considerable period; and it will be therefore necessary, from time to time, to refer the line of collimation of the telescope to the meridian, by means of the transit instrument. This observation may be repeated *once a month*, or more frequently if any change in the position of the telescope be suspected.

In the case of the *intensity*, there is another source of error, (besides that due to a change in the position of the instruments) which can only be guarded against by a repetition of *absolute* measurements. The magnetic moment of the magnet itself may alter; and the observations of intensity changes

afford no means of separating this portion of the effect from that due to a change in the earth's magnetism. This separation can only be effected by means analogous to those employed in the determination of the absolute value of the horizontal intensity; and accordingly one or other (or both) of the methods proposed for this determination should be occasionally resorted to. It is desirable that this observation should be repeated *once in every month*; and more frequently, whenever the changes observed with the horizontal force magnetometer indicate, by their *progressive* character, a change in the magnetic moment of the suspended bar.

It would be easy, in theory, to devise a method by which the vertical force magnetometer might be made to serve in determining the absolute value of the vertical intensity. The means which at present offer themselves appear, however, to be surrounded with practical difficulties; and it seems safer to deduce this result *indirectly*. From the formulæ given in page 452, we have

$$Y = X \tan \theta;$$

so that if the *inclination* θ be known, and the horizontal intensity X determined in absolute measure, the vertical intensity Y is inferred.

For the purpose of observing the element θ , each observatory is furnished with an inclination instrument, the circle of which is $9\frac{1}{2}$ inches in diameter. The observation should be made in an open space, sufficiently remote from the magnets of the observatory, and from other disturbing influences; and a series of measures should be taken *simultaneously* with the two intensity magnetometers, for the purpose of eliminating the *changes of the inclination* which may occur in the course of the observation. As to the mode of observation, the best seems to be the usual one, the plane of the circle coinciding with the magnetic meridian; but for the purpose of testing the axles of the needles, and the divided limb of the instrument, it is desirable that some observations should be made in *various azimuths*,—for example, every 30° of the azimuth circle commencing with the magnetic meridian. The inclination is then inferred, from each pair of corresponding results, by the formula

$$\cotan^2 \theta = \cotan^2 \zeta + \cotan^2 \zeta';$$

ζ and ζ' being the observed angles of inclination in two planes at right angles to one another. Where the inclination is great (as at Montreal,) this method will serve to test only a limited portion of the circumference of the axle and limb. In this

case the best course appears to be that pointed out by Major Sabine,* namely, to convert one of the needles, temporarily, into a needle on Mayer's principle, by loading it with sealing-wax; and to deduce the inclination, from the angles of position of the loaded needle, by the known formula of Mayer. The observations here suggested having been very carefully made, and the inclination changes eliminated in the manner above explained, the observed difference between the *mean* and the result obtained in the *magnetic meridian*, should be applied as a correction for the errors of axle and limb to all future observations made in the meridian.

These observations should be made at the same periods as those of the absolute horizontal intensity,

VARIATION OF THE ELEMENTS.

The *variations* of the magnetic elements are, 1. Those variations whose amount is a function of the *hour angle* of the sun, or of his *longitude*; and which return to their original values at the same hour in successive days, or the same season in successive years. These, from their analogy to the corresponding planetary inequalities, may be denominated *periodical*. 2. The variations, which are either, continually *progressive*, or else return to their former values in long and unknown periods; these may in like manner be denominated *secular*. 3. The *irregular variations*, whose amount changes from one moment to another, and which observe (apparently) no law.

The *periodical* variations (with the exception of those of the *declination*) have hitherto been little studied; and, even in the case of the single element just mentioned, the results have scarcely gone beyond a general indication of the hours of maxima and minima, and of the changes of their amount with the season. The subject is nevertheless of the highest importance in a theoretical point of view. The phenomena depend, it is manifest, on the action of solar heat, operating probably through the medium of thermo-electric currents induced on the earth's surface. Beyond this rude guess, however, nothing is as yet known of the physical cause. It is even still a matter of speculation whether the solar influence be a *principal*, or only a *subordinate* cause, in the phenomena of terrestrial magnetism. In the former case, the periodical changes are to be regarded as the effect only of the

* Reports of the British Association, vol. vii. p. 55.

variations of that influence; in the latter, they must be considered as its entire result, the action in this case only serving to modify the phenomena due to some more potent cause. It may be fairly hoped that a diligent study of this class of phenomena will not only illustrate this and other doubtful points in the physical foundation of the science; but also, whenever that physical cause shall come to be fully known, and be made the basis of a mathematical theory, the results obtained will serve to give to the latter a numerical expression, and to test its truth. Even the knowledge of the empirical laws of the hourly and monthly fluctuations must prove a considerable accession to science; and (as one of its more obvious applications) will enable the observer to reduce his results, as far as this class of changes is concerned, to their *mean* values.

For the complete determination of the hourly and monthly changes of the magnetic elements, a persevering and labourious system of observation is requisite. The *irregular* changes are so frequent, and often so considerable, as (partially at least) to mark the regular; and the observations must be long continued at the same hours, before we can be assured that the irregularities do not sensibly affect the mean results. Again, in a theoretical point of view, the nocturnal branch of the curves by which the periodical changes are represented is quite as important as the diurnal; and it is manifest that nothing can be done towards its determination without the co-operation of a number of observers. At each of the observatories about to be founded by the liberality of Her Majesty's Government, there will be three assistant observers placed under the command of the director; and it is intended that the observations shall be taken *every two hours* throughout the twenty-four. In order that this series of observations, which is especially destined for the determination of the periodical changes, may at the same time cast some light upon the irregular movements, it is proposed that they shall be *simultaneous* at all the observatories. The hours which have been agreed upon are the *even* hours (0, 2, 4, 6, &c.) *Göttingen mean time*. It is likewise intended that *one* observation of the twelve shall be a *triple* observation, the position of the magnets being noted *five minutes before and after* the regular hour. The time of this triple observation will be 2 P.M., Göttingen mean time.

The barometer, and the wet and dry thermometers, will be registered at each of the twelve magnetic hours. No observation will be taken on Sunday.

No distinct series of observations is required for the deter-

mination of the *secular* variations. In the case of the *declination*, the yearly change will be obtained by a comparison of the monthly mean results (for the *same month* and *same hour*) in successive years. The observations of two years only will thus furnish 144 separate results, from which both the periodical and the irregular changes are eliminated; so that great precision may be expected in the final result, notwithstanding the limited period of observation. The same mode of reduction will apply to the two components of the *intensity*, provided that no change shall have taken place in the magnetic moment of the bars employed. In the latter event, recourse must be had to the *absolute* determinations for a knowledge of the secular changes.

The subject of the *irregular* movements has acquired a prominent, and almost absorbing interest, from the recent discoveries of Gauss. It has been ascertained that the resultant direction of the forces, by which the horizontal needle is actuated at a given place, is *incessantly* varying, the oscillations being sometimes small, sometimes very considerable:—that similar fluctuations occur at the most distant parts of the earth's surface, at which corresponding observations have been as yet made;—and that the instant of their occurrence is the same every where. The intensity of the horizontal force has been found subject to analogous perturbations.

For the full elucidation of the laws of these most interesting phenomena, it is of the first importance that the stations of observation should be separated as widely as possible over the earth's surface, and that their positions should be chosen near the points of maxima and minima of the magnetic elements. This has been in a great measure accomplished as regards the observatories about to be founded by Her Majesty's Government. The stations are wide asunder in geographical position, and they are in the neighbourhood of points of prominent interest in reference to the isodynamic lines. The results of observation at these stations will soon testify whether the shocks to which the magnetic needle is subject, are of a local or of a universal character as regards the globe; and in either event we may expect that they will furnish information of great value (in reference to a physical cause) as to the magnitude of the phenomena in different places, and the elements on which it depends.

In the observations destined to illustrate these phenomena, it is proposed to follow, as nearly as possible, the plan laid down by Gauss. One day in each month, namely, the *last*

Saturday, will be devoted to simultaneous observations on this system; the observations commencing at 10 P.M. of the preceding eve (Göttingen mean time,) and continuing through the 24 hours.

LXIX.—*On Electro-Magnetic Forces.* By J. P. JOULE, Esq.

I. About the commencement of last April, I made some experiments in electro-magnetism, which I had the pleasure of communicating to the readers of this excellent work, in two letters to the Editor, dated on the 28th of May, and on the 10th of July. I am desirous of making some additional observations on that subject, especially as subsequent experience has enabled me to place in a more correct view some of the effects I then witnessed.

2. I have shown* that when a current of voltaic-electricity is transmitted through the coils of two electro-magnets, their mutual attraction is in the ratio of the squares of the quantities of electric force: and also that the lifting power of the "horse-shoe" electro-magnet is governed by the same law.

3. I have recently made experiments which prove that the attraction of the electro-magnet, for a magnet of constant force, varies in the simple direct ratio of the quantity of electricity passing through the coil of the electro-magnet. (In order to succeed, it is necessary to guard against the effects of induction by a proper arrangement of the apparatus.)

4. Magnetism appears therefore to be excited in soft iron, in the direct ratio of the magnetizing electric force; and electro-magnetic attraction, as well as the attraction of steel magnets, may be considered as proportionate to the product of the intensities of each magnet, or, which is the same thing, to the number of lines which may be drawn between the several magnetic particles of the attracting bodies.

5. This view is illustrated by figs. 1, 2, and 3, Plate XI. where the several attractions of the magnetic particles, viz. 1 to 1, 2 to 1, and 2 to 2 are represented by the number of lines drawn in each instance, 1, 2, and 4.

6. I have recently understood, that the Russian philosophers Jacobi and Lenz, have arrived by their experiments, at some of the same conclusions with regard to the laws of

* Vol. 4, page 134—135.

electro-magnetic attraction. I have not read their papers, but shall be most happy if they shall be found to confirm the results of my observations.

7. Fig. 4, will perhaps illustrate, with a considerable degree of accuracy, the complex action of the forces which constitute the aggregate attraction which exists between two magnets, for instance, *A*. and *B*. The magnetic particles of which six only *a*, *b*, *c*, *d*, *e*, *f*, are drawn, may be conceived to be of an indefinitely large number spread throughout the region of the "poles;" and the several forces are represented by the straight lines drawn been those particles.

8. If this view be correct, it is obvious that the closer the approximation of the magnetic particles in each system, the greater will be the magnetic attraction; for in that case the particle *a* will both be nearer the particle *f*, and the force exerted between them will be in a less oblique direction.

9. It was in consequence of my entertainment of a different hypothesis, that I was led to imagine that I had detected a decrease of power on increasing the length of the electro-magnet; in Vol. 4, page 136 and 137, is a comparison of the powers of three electro-magnets of the several sizes, $\frac{1}{11}$ $\frac{1}{11}$ and $\frac{1}{11}$, of an inch square, with those of two whose sectional areas were respectively, one inch square, and one inch by 2 inches. It is probably in a great measure the consequence of the principle, (7) that the mean power of the latter, was found to be less than that of the former, in the ratio of 7000 to 10646; and this observation is further corroborated by the fact, that, of the long electro-magnets the less powerful has the more extended "pole." It would, however, be a matter of no difficulty to determine the influence of length on magnetic conduction.

10. Hence also a correction should be applied to the attractions of the larger electro-magnets* in order fairly to compare their respective powers with those of smaller dimensions. I will not venture to decide its amount, as that will be entirely dependant upon the distances of the polar particles, (7), from the ends of the electro-magnets; if $\frac{1}{2}$ were added to the attractions opposed to Nos. 5, they would I think, be placed in pretty correct comparison with Nos. 1.

11. These corrections are not however of sufficient amount to affect the general conclusion to which I have come, with regard to the laws under which magnetic attraction, (as applicable to the production of motive force,) is developed by electricity, viz.: *That the attraction of two electro-magnets*

* Annals Vol. 4, page 133.

towards each other, is in every case represented by the formula $M = W^2 E^2$, where M , denotes the magnetic attraction, W , the length of wire, and E the quantity of electricity conveyed by that wire in a given period of time; a formula modified merely by the effects, of saturation, of the conducting power of iron, and of the distance of the coils from the surface of the iron.

12. I have observed, that magnetic and electro-magnetic attraction decreases, in certain cases, in the simple ratio of the distances. This was found to be particularly the case when the magnets were long, and the distances between them small. Mr. Harris has observed the same effect, see his "Experimental Inquiries concerning the laws of Magnetic Forces." It may be principally accounted for by the complex action previously illustrated. It is impossible to doubt that the law of magnetic attraction is inversely as the squares of the distances.

13. I shall now in accordance with my promise enter into the detail of some experiments with the electro-magnetic engine described in the "Annals" for October, 1839; and first it will be proper to describe the apparatus I had occasion to use.

14. The galvanometer was constructed on the plan which was described in pages, 131 and 132, of the present volume. The coil is rectangular, 12 inches long and 6 inches broad; the copper wire is $\frac{1}{12}$ of inch thick, and the length of the needle rather less than 4 inches. To make it more extensively available, I have drawn a curve, whose abscissæ are the degrees of the circle, and whose ordinates are the quantity numbers corresponding to those degrees, in this way I can interpolate to any extent the quantity divisions previously obtained by experiment.

15. I recommend this form of the galvanometer with great confidence, because, 1st, The method of tangents is only applicable when the diameter of the coil bears a very large ratio to the length of the needle, and 2nd, Because you can by passing the electric current through 1, 2, 3, 4, &c. coils, increase the delicacy of the instrument accordingly.

16. I have principally made use of Wollaston's 4 inch doubly coppered batteries, with amalgamated zinc plates, and charged with a solution of sulphuric acid. I shall perhaps describe at an early opportunity an expeditious and convenient method of fitting up both this battery, and the admirable instrument of Mr. Grove.

17. In the subsequent experiments, the engine was fitted up with the hard iron, and hard wire, revolving electro-magnets. After a few trials with powerful batteries, finding

it impracticable to work with the highest intensity arrangement, I soldered the ends of the three wires of each electro-magnet together, and united the combined wires in such a manner, that the electric current passed through 424 yards of threefold conducting wire.

18. In the tables underneath, the first column indicates the quantity of electricity; the second, the differences of those quantities; the third, the velocity of the revolving electro-magnets, in feet, per second; the fourth, the duty, in pounds raised per second of time, to one foot in height; and the fifth, the duty, in pounds raised to the height of one foot by the agency of one pound of zinc.

19. In calculating the amount of duty, I found that in this arrangement, 12·4 of electricity was just sufficient to keep the machine in motion, when the friction, referred to the revolving electro-magnets, was equal to 10 ounces avoirdupoise; the same amount of electricity was, whatever the velocity, always able to overcome exactly the same amount of friction; I therefore felt justified in making it a basis on which to calculate the force due to other quantities of electricity. The duty in the fifth column is calculated on the basis of the decomposition of water effected by a given quantity of electricity; I consider it as an *approximation* to the truth. I may just observe that the friction has been altogether neglected, and that whenever the motive force was not sufficient, mechanical means were resorted to in order to overcome it; this course was adopted, because the friction is not at all to be considered as an element in the subsequent observations.

TABLE 1.

80 pairs of Wollaston's plates.

(A mean of 3 trials.)

Electricity.	dif.	Velocity.	Duty.	Economical duty.
24·6.....	0.....	0.....	0.....	0
	3			
21·6	2	3·8	21960	
	2			
19·6	4	6·25	39740	
	1·6			
18·0	6	7·89	54800	
	1·5			
16·5	8	8·85	66950	
	1·5			
15·0	10	9·15	76140	

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TABLE 2.

40 pairs of Wollaston's plates.

(A mean of 2 trials.)

Electricity.	dif.	Velocity.	Duty.	Economical duty.
11·8.....	0.....	0.....	0	
	1·6			
10·2.....	2.....	·85.....	20700	
	·8			
9·4.....	4.....	1·44.....	38300	
	·8			
8·6.....	6.....	1·80.....	52320	
	·6			
8·0.....	8.....	2·08.....	65000	

TABLE 3.

10 pairs of Wollaston's plates.

5.....	0.....	0.....	0	
	·8			
4·2.....	2.....	·14.....	33300	
	·6			
3·6.....	4.....	·21.....	58300	
	·3			
3·3.....	6.....	·265.....	80300	
	·3			
3.....	8.....	·292.....	97300	

TABLE 4.

Grove's battery of 10, 4 inch, plates.

(Not very efficiently charged.)

17·6.....	0.....	0.....	0	
	3·3			
14·3.....	2.....	16·6.....	116080	
	1·9			
12·4.....	4.....	2·5.....	201600	
	1·4			
11·0.....	6.....	2·95.....	268200	
	·8			
10·3.....	8.....	3·38.....	331400	

20. I now united the conductors in such a manner, that the fluid was divided between each pair of stationary, and revolving, electro magnets; in this case, the electricity passed through 212 yards of six-fold wire.

TABLE 5.

A quantity arrangement of two 40 pairs of Wollaston's plates.

Electricity.	dif.	Velocity.	Duty.	Economical duty.
52	—	0	0	0
	9			
43	—	2	3.76	21800
	5			
39	—	4	5.87	38600
	3.2			
34.8	—	6	7.38	53100
	2.6			
32.2	—	8	8.42	65400
	2.4			
29.8	—	10	9.02	75700

TABLE 6.

A quantity arrangement of two 20 pairs of Wollaston's plates.

28.2	—	0	0	0
	5			
23.2	—	2	1.1	23700
	2.5			
20.7	—	4	1.74	42000
	1.7			
19.0	—	6	2.205	58000
	1.4			
17.6	—	8	2.52	71600

TABLE 7.

A quantity arrangement of two 10 pairs of Wollaston's plates.

16.8	—	0	0	0
	3			
13.8	—	2	.387	28000
	1.6			
12.2	—	4	.605	49600
	1.2			
11.0	—	6	.738	67100
	1.0			
10.0	—	8	.813	81300

21. The above examples will show pretty clearly the effects of magnetic electrical resistance. This resistance is the prime obstacle to the perfection of the electro-magnetic engine, and in proportion as it is overcome, in the same proportion will the motive force increase; this ought therefore to claim our first attention.

22. On comparing the *differences* with the *velocities* and *electricities* in each table, the general conclusion is, that *the magnetic electrical intensity is directly proportional to the velocity, multiplied by the magnetism*, or, which is the same thing, by the electricity which induces that magnetism. It is the latter part of this law, which makes the *differences* decrease generally, (and as accurately as the nature of the manipulations can lead one to expect,) in the same ratio with the *electricities* opposed to them. It is necessary to observe that the *first difference*, or that which exists between 0, and 2, *velocities*, must be neglected, as that is much augmented by the slightest inaccuracy of the commutator.

23. It appears moreover, that this law is *entirely unaffected* by the diminution or increase of battery intensity; for on comparing the tables of either system together, it will be seen that in all cases the *differences* are about one-tenth of the *electricities* opposed to them. I wish to call particular attention to this circumstance, which is owing to the constant resistance of the wires, in each separate system.

24. In the second arrangement the conducting metal was half as long and twice as substantial as it was in the first; hence it is, that half the battery intensity sufficed to pass twice the quantity of electricity, and so to produce the same motive effect. This is seen on comparing table 1, with table 5.

25. Also, on referring to tables 1 and 5, it will be observed, that the *differences* are twice as great in the 2nd arrangement as in the 1st, whilst the magnetic force remained very nearly the same. To understand the reason of this, it will be necessary to observe, 1st; that the magnetic electrical intensity has nothing whatever to do with the thickness of the wire upon which it is induced, but exists *simply in the direct ratio of the length*, consequently that the intensity is only one half as great in the 2nd arrangement, as it is in the 1st; And 2nd, that, as the resistance of the wire to the battery current, in the 2nd arrangement, is only one quarter of that in the 1st, the *same* additional, or extraneous, resistance will produce four times the effect in the former, as in the latter instance. Hence by compounding these two effects, we have the differences of electricity, due to a given increment of

velocity, and the same amount of magnetism, twice as great in the 2nd, as in the 1st arrangement.

26. If the intensity of the voltaic battery do not increase in a less ratio than that of the number of its pairs, there will theoretically, be no variation in economy, whatever the arrangement of the whole conducting metal, or whatever the size of the battery. For, if the battery be doubled in intensity, it must in that case consist of twice the number of pairs, which will cause twice the quantity of electricity to pass, and hence four times the weight of battery materials will be consumed, while the force of the engine is also increased four times, according to the square of the electricity. See the economical duty in the tables 1, 2, 5, and 6.

27. The following are three resources on which to rely, in order to obtain economical power; 1st, the increase of the quantity of conducting wire, which will produce a *variable* degree of advantage, for while it diminishes the resistance of the wire, it produces no effect upon the magnetic electrical resistance; 2nd, the augmentation of the intensity of the elementary battery, which will produce an exactly similar increase of duty: (compare table 3 with table 4.) 3rd, the improvement of the arrangement of the electro-magnets. Had I placed mine in such a position that the *broad* edges of the poles should have acted on each other, I should doubtless have attained a considerable higher amount of duty.

28. I must apologize to the reader, that I have not relieved the tediousness of this paper, by a single brilliant illustration. I have neither propelled vessels, carriages, nor printing presses. My object has been, first to discover correct principles, and then to suggest their practical development. If I have succeeded in some measure in the first part of that object, my design has been fully realized.

Broom Hill, near Manchester,

March 10th, 1840.

LXX. *Wonderful effects of Voltaic Electricity in restoring Animal life when the sensorial powers have entirely ceased or in other words, when death in the common acceptation of the term has actually occurred.* Extracted from Mr. W. H. HALSE's address to the Newton Society for the attainment and diffusion of Knowledge dated March 3, 1840.

After describing the benefits obtained by a study of the sciences generally, he thus proceeds to show the powers of

galvanism on the animal body.

“On Thursday last one of my spaniels whelped, having a litter of thirteen; six of which I took for my experiments. I drowned three of them in *cold* water and kept them immersed for fifteen minutes, at which time I took them from the bucket and placed them in front of a good fire;—*no motion could be perceived in either of them.* I then put the front legs of one of them in a jar containing a warm solution of salt and water and its hind legs in a similar jar, in each of which was inserted one pole of the galvanic battery; the whole was then placed near the fire.

“The position of the dog being now favourable for operating on, without the necessity of making any incisions in the flesh, I passed a very strong shock through its body; it moved its hind legs; I gave it another shock, which caused its tail also to move; I now passed twenty shocks in quick succession through its body: *it moved every limb, its mouth opened and I was inclined to believe that the dog had actually come to life*; but the moment I ceased passing the shocks, the dog was as motionless as it was previous to my commencement. Again I continued the shocks and I noticed that there was more motion in the limbs:—considering that in proportion to the return of sensibility, that these shocks would be too powerful for it, I decreased the *intensity* of them and passed many hundreds in rapid succession; I continued this for about five minutes—the motion of the limbs increasing as the shocks increased in number—I now ceased; *the dog still moved.*—IT WAS RESTORED TO LIFE.—I placed it on a warm flannel in front of the fire and in a very short time it appeared as well as it was previously to its being drowned; it crawled on the flannel and made the noise peculiar to young dogs. I now examined the two other dogs which were drowned and taken from the water at the same time that this one was.—THEY WERE BOTH DEAD—a *plain proof that it was entirely owing to the galvanic fluid that life was restored.*

“The other three dogs I drowned in *warm water* and kept them immersed for *forty minutes*, at which time all motion had ceased; two of them I laid in front of the fire and the remaining one I placed in the jars as in the preceding experiment. I now passed a few shocks of weak intensity through the body, but no motion was perceptible; I therefore increased the intensity of them considerably and gave the shocks in quick succession.—*Every limb moved, the belly protruded and again collapsed, and the head was raised*—at this period I stopped passing the shocks in order to see if there

were any motion in the dog when not under the galvanic influence ;—there was none ; I again proceed with the shocks and having noticed that the limbs moved more rapidly than before, I considered it necessary to decrease the intensity and increase the *quantity* of electric fluid, which I did so much as just to be enabled to perceive a slight tremor in the dog ; I continued in this manner for about five minutes at which time I removed it from the jars and placed it on the table.—It was ALIVE.—In a quarter of an hour it appeared to be perfectly recovered. The other two dogs (which were not allowed to get cold during the whole of the experiment) were now examined ; *no motion whatever could be perceived*. I tried the effect of galvanism on one of these ; I was successful. In one hour after this I operated on the other dog also ; *but 'twas in vain—there was no vigour remaining in the vital powers ;—life had fled*.

“Remarks:—Having stated that I restored the dogs to life, it will be necessary for me to explain in what light this is to be understood. Strictly speaking, life was not extinct in either of the dogs previously to my operating, for if it had they would certainly have remained dead ; *it was merely a cessation of the sensorial functions, whilst there was a degree of vigour still remaining in the vital organs which combined with the nervous influence (or a substitute for it if you please) I supplied by the powers of galvanism ; were sufficient to restore these functions to their former state of activity ;* nevertheless, the dogs were in the common acceptance of the term—dead ; but not properly so, for death cannot be considered to have actually arrived until the sensorial, the muscular and the nervous functions all cease to act—at that moment the animal dies and not before. We therefore see that although the dogs were not strictly speaking—dead, previous to my operations, yet by the fact of the others being dead when the sensorial of those three were restored, it must be evident that the process of dying had commenced, *and would have been perfected had not the powers of galvanism been resorted to ;*—therefore when I say the dog was restored to life, I must not be considered to mean that I brought the dead to life but rather that I arrested the process of dying, by restoring the sensorial functions—which functions had before entirely ceased to act.

“The nature of the above experiments must be very familiar to every physiologist, but when we consider the astonishing powers of galvanism on the human frame, in supplying the nervous fluid (or a substitute for it) and the ignorance of

this fact by a large proportion of the medical profession; perhaps I shall be excused for introducing this subject to your notice; and as the apparatus necessary for the purpose, when constructed on my principle is quite unexpensive (one guinea, see No. 23 of Sturgeon's Annals of Electricity, Magnetism, and Chemistry,) *I hope there will not be found many medical practitioners who will object to add the powers of voltaism to their other modes of resuscitation from the first stage of death caused by drowning, or from that caused by suffocation through noxious gases.*

"I have refrained from introducing many technical terms, as I wish my subject to be generally understood. It will be perceived, that in passing the shocks through the bodies of the dogs, no cruelty was practised, for when the powerful shocks were passed, they possessed no feeling whatever, and in proportion as the sensorial powers and consequently the feeling returned, the intensity of the shocks were reduced; and when it is also considered that the dogs would have been drowned had these experiments not been made, I trust I shall not be accused of having had recourse to cruel methods, for the purpose of putting the powers of the voltaic electricity on the animal body, to the test of experiment."

WILLIAM H. HALSE.

Brent, near Ashburton.

LXXI. *On Lightning Conductors, and the effects of Lightning on Her Majesty's Ship Rodney and certain other Ships of the British Navy; being a further examination of Mr. Sturgeon's Memoir on Marine Lightning Conductors. By W. SNOW HARRIS, Esq. F.R.S., &c,*

To the Editors of the Philosophical Magazine & Journal.

GENTLEMEN,

1. In my former communication (L. and E. Phil. Mag. vol. xiv. p. 461.) I considered the nature of a well-known phenomenon in electricity, termed by Cavallo, Priestley, and others the lateral explosion, and shewed that it did not apply to the state of a metallic rod in the act of transmitting a vanishing electrical accumulation between two opposed electrified surfaces, as insisted on by Mr. Sturgeon in a recent number of his Annals of Electricity. I will now proceed to examine the general character and effect of ordinary electrical dis-

charges, whether produced on the great scale of nature, or artificially, with a view of further showing, that such lateral explosions do not occur at the instant of the passing of a shock of lightning through a metallic conductor, as also with a view of meeting certain other objections which have been advanced at different times to the use of lightning rods in ships.

2. I should not have felt myself called upon to notice further Mr. Sturgeon's memoir, did I not consider the statements it contains, although superficial and inconclusive, likely to mislead the public upon many important points, connected with the effectual protection of shipping, against the destructive effects of lightning, and convey false views of the nature of electrical action. Under these impressions I have little hesitation in noticing what he has advanced under the following heads:—

1st. Examination of the observed effects produced on shipping by lightning.

2nd. A comparison of the observed effects of lightning and the probable effects which lightning would produce by the application of Mr. Harris's conductors to shipping.

3. The first contains an excellent, and I have no doubt, an accurate statement, by an intelligent officer of the *Rodney*, of the destructive effects of lightning lately experienced in that ship, together with notices of two cases in which ships fitted with my conductors were struck by lightning without any attendant ill consequence. In the second, it is the author's object to prove, from the effects of lightning in the *Rodney*, that my system is inadmissible; since the discharge of lightning, he observes, which struck the *Rodney*, "would have been powerful enough to have rendered even the thickest part of Mr. Harris's conductors sufficiently hot to ignite gun-powder.

Considering the boldness of this assertion, and the high pretension of the memoir, we should expect, on examining the author's researches, to find him in possession of a copious induction of facts from well-authenticated cases of damage by lightning on ship-board, illustrating clearly the views he so strenuously insists on,—cases in which continuous or other metallic conductors have been from any cause placed along the masts or rigging, and in which the electric agency found its way through the hull to the sea. We should further expect from him, something like an examination of the general nature and effects of electrical discharges, since it is clear before any accurate estimate can be arrived at, of the relative quan-

tity of electricity likely to be discharged from a thunder-cloud, and the probable effects of metallic rods, or other conductors set up with a view of directing it in any given course, such information is quite indispensable.

4. Now it is to be particularly observed, that Mr. Sturgeon's memoir is really deficient in such information; a few clumsy experiments in illustration of a well-known fact in electricity, deceptively associated, by means of a vague hypothesis, with some of the ordinary effects of lightning, on a ship *not having* any regular conductor, and with some every-day phenomena of the electrical kite, is virtually the amount of all that the author has advanced, under the imposing title of "Theoretical and Experimental Researches.

5. In illustration of the careless way in which he meets this question, it may not be out of place to notice the following specimen of his inductive philosophy,—being the very outset of the comparison he has proposed, of the observed effects of lightning, and the probable effects on my conductors*.

In the account given of the damage recently sustained by H.M. Ship Rodney, it appears, that the shock of lightning which shivered the top-gallant-mast, damaged the top-mast, &c., &c., fell on a small brass sheave in the truck for signal halliards, and *slightly* fused it. This sheave weighed about four ounces; it was only about an inch and a half diameter, hollowed except at the centre and rim, where it was somewhere about half an inch in thickness. The lightning also fell on the copper funnel for top-gallant rigging, being a hollow cylindar of sixteen inches in length, 10 inches in diameter, and not quite a quarter of an inch thick. This funnel was not anywhere fused. It fell also on other metallic masses, such as the iron-bound tie-block, on the top-sail-yard, &c., &c., the iron hoops of the masts, &c., on which no calorific effect was apparent.

6. Now we have here something like evidence what was really the *actual power* of the charge. We see, for example, that it *did not* fuse a copper funnel, 16 inches long, 10 inches in diameter, and about 1/4 of an inch thick. In the face of which fact Mr. Sturgeon insists, that had the charge fallen on my conductor, the thickest part of it would have become red-hot. His reasoning, in fact, amounts to this; an explosion of lightning having *slightly* fused a small brass sheave, weigh-

* Sturgeon's Memoir, sec. 204.

ing 4 ounces, and having failed to fuse a short copper funnel, therefore had it fallen on a rod of copper of one inch in diameter, and 200 feet long*, that rod would have been rendered *red-hot*.

This, it must be allowed, is a somewhat amusing kind of special pleading, quite unprecedented, I believe, in any paper on science.

7. The author wishes to strengthen his deduction, such as it is, by adverting in a foot-note to the case of a small brig struck by lightning, in which some part of a chain conductor is *supposed* to have been fused; how much is not known, "as the lower part fell overboard." The statement is given without any quoted authority, and is altogether deficient in the very information most required, viz. the *size of the chain, and how much of it was fused*. Let us, however, take it upon the author's own ground, and suppose the conductor to have been such as is commonly used in the merchant service,—that is to say, links of iron wire about one-fourth of an inch in diameter, united by rings, a kind of conductor very easily disjointed and fused at the points of junction by lightning;—the reasoning then stands thus: because a shock of lightning fused and disjointed some unknown portion of a lightning chain in a merchant brig, therefore the same shock, had it fallen on a solid copper rod of one inch in diameter and 100 feet long, would have rendered that rod *red-hot*.

8. The fallacy and entire worthlessness of such reasoning, seems not altogether to have escaped Mr. Sturgeon's notice, as appears by his amplification of the above effects; thus on entering upon the comparison of the effects of lightning, he resorts to a sort of wholesale dealing, and leads the reader to conclude that the *entire* sheave in the Rodney and *all* the brigs' conductor underwent fusion. But even if it were so, no such conclusion as that above mentioned is admissible†, especially in reference to a continuous and massive conductor

* This is the equivalent of my conductor on the main-mast of such a ship as the Rodney, taking it at its least value.

† "Were there no other data than those of the *fusion of the metallic sheave* in the Rodney and the *fusion of the chain conductor* in the brig Jane," &c. &c.

"The impressions which these facts convey to the mind are too definite to be easily understood; they clearly imply that either of the discharges which struck the Rodney or Jane would have rendered the thickest part of Mr. Harris's conductors sufficiently hot to ignite gunpowder," &c. &c.—Sturgeon's Memoir, sec. 204.

terminating in a point, and equalizing with inconceivable rapidity the disturbed electrical state of the sea and clouds.

9. The manifest deficiency of sound practical information in Mr. Sturgeon's memoir, imposes upon me the necessity of adverting to the general character and operation of common electrical discharges, whether produced by artificial means or on the great scale of nature. In doing this I have no desire to excuse myself, in case I should not have written clearly and explicitly on the subject, since in no department of physics is the field of observation so fertile, and the path of experiment so sure and easy. We have before us the experience of nearly a century, during which time lightning-rods have been employed; a great number of instances have occurred of shocks of lightning falling on ships under a variety of different circumstances, in some cases where lightning conductors have been present, in others where absent; in many instances where ships have been near each other and exposed to the same storm, some *having* conductors, others *not*. The general laws of the discharge are traceable in them all, and the effects on metallic bodies distinctly shown. On the other hand, we can on a minor scale, imitate successfully the great operations of nature, and examine experimentally every possible contingency attendant on the operation of a shock of lightning in a ship. It is our own fault, therefore, if we do not treat the subject scientifically, and arrive at complete practical solutions of such questions as these: Is a lightning conductor desirable in a ship? Will it cause by attraction a shock of lightning to fall on a ship when otherwise such would not take place? If so, can it cause damage by its inability to get rid of the lightning which falls on it? What is the *best* form and dimensions of a lightning conductor for a ship? What is the greatest probable force of lightning to which it may become exposed? Is it liable to cause damage by any lateral operation of the charge passing through it? I say, if such questions as these cannot now be reasonably determined they probably never can; and, therefore, any one who writes or reasons obscurely about them, and without due regard to a good induction of facts, can have no claim to be considered as a sound reasoner in experimental science; for, as beautifully observed by Lord Bacon "Man, who is the servant of nature, can act and understand no further than he has, either in operation or in contemplation, observed of the method and order of nature." Under these impressions I proceed to examine the general character and effects of electrical discharges as exhibited artificially, and on the great scale of nature.

10. Although some theoretical differences may have arisen concerning the precise nature of electricity, yet the following explanation runs sufficiently parallel with facts to entitle it to our confidence, and put us in possession of one of the great advantages of *every* theory, viz. a classification and connexion of observed effects; the province of human knowledge, being, as justly observed by a most intellectual and accomplished writer, "to observe facts, and trace what their relations are."*

General principles:—

11. There is an invisible agency in the material world intimately associated with common matter, termed electricity.

12. Lightning, thunder, and a variety of analogous phenomena of a minor kind, artificially produced, result from discharges of this agency between bodies differently affected by it.

13. In every case of electrical discharge there are two surfaces of action; one existing on some substance eager to throw off redundant electricity, being, according to Dr. Franklin, overcharged with it; the other existing in some other substance eager to receive electricity, being, according to the same philosopher, deficient of it, or undercharged.

14. Two opposed bodies, when placed in these opposite electrical states, have a sort of exclusive action on each other, either *directly* through any intervening substance, whether a conductor of the electrical principle or not, or otherwise *indirectly* through any lateral circuit.

Thus two metallic surfaces A B (fig. 1, plate X.) pasted on the opposite sides of a square of glass *c d*, have, when the square is said to be charged, an exclusive action on each other, either through the intervening glass, or otherwise through any conductor, A o B, connecting them.

Now we have only to suppose these planes placed further apart, as in fig. 2, to have a discharging conductor, *m n*, of greater or less extent between them, to be greatly increased in size, to be separated by air instead of glass, and to consist of free vapour or water, and we have a pretty faithful representation of the conditions, under which a discharge of lightning takes place, when passing partly through the air, and partly through a discharging conductor, *m n*, or any other body, *c d*, placed on the plane B.†

* Abercrombie on the Intellectual Powers.

† The thickness of the intervening air, and the amount of free electricity in the clouds, has led Professor Henry to question in some measure, the

15. Any continuous metallic rod or other body, $m n$, (fig. 2,) connected with the lower plane, must be considered merely as a passive way of access for the charge so far as it goes; the electrical agency being observed to seize upon substances best adapted and in a position to facilitate its progress, or otherwise to fall with destructive effect upon such as resist it.

16. It is easy to perceive here, that the presence of a conducting rod, $m n$ (fig. 2,) or other conducting body, has nothing whatever to do with the great natural action set up between the planes $A B$. It is in fact to be considered merely as a point in one of them. The original accumulation of electricity and subsequent discharge, would necessarily go on whether such a rod were present or not, as is completely shown by experience. When present, its operation is confined to the transmission, so far as it extends, of that portion of the charge which happens to fall upon it; and since it is quite impossible to avoid the presence of conducting bodies in the construction of ships, it is the more important to understand clearly in what way damage by lightning occurs to the general mass, and how it may be best avoided.

17. When discharges of lightning fall upon a ship in the way above stated, as being a heterogeneous mass fortuitously placed between the charged surfaces $A B$ (fig. 3.), the course of the discharge is always determined through a certain line or lines, which upon the whole least resist its progress. The interposed air between the ship and the clouds first gives way in some particular point, probably the weakest,—suppose at

perfect analogy of a discharge of lightning, with that of a Leyden jar; but I think upon *mature* consideration this circumstance will not be found in any way subversive of the general principle. Thus whether electricity be accumulated on thick glass or on thin, the result is the same; it is merely the intensity as indicated by the electrometer which changes.

Now the term free electricity, applies to the greater or lesser influence of the opposed coating in respect of other bodies. In the case of the opposed surfaces of the clouds and earth, all the charge is necessarily free electricity, since there exists no other point upon which it can tend to discharge. In the same way the electricity of the jar, when the coatings are very near, is nearly all redundant, or free electricity, in respect of the action between them, although latent in respect of other bodies. Hence with a moderate accumulation, the electrometer exhibits but a small intensity, if any. The only difference at the time of the discharge, is in the position of the discharging circuit, which in the case of the clouds and sea, is directly in the interval of separation; and as we find the principal of induction always active in cases of lightning, the thickness of the stratum has evidently no influence on the conditions of the accumulation, especially when we consider the great extent of the opposed surfaces, which may possibly be 20,000 or more square acres. Dr. Faraday has shewn that no distance excludes the the inductive action.

A, fig. 3;—the electrical agency then meeting with continued resistance from the non-conducting particles of air, is often turned into a tortuous course. Suppose it arrives in this way at some point, m , in the vicinity of a ship at k , the question whether it would strike upon the mast at y would be determined by the resistance in the direction of $m y k$, as compared with that in any other direction m, B ; whether, in fact, it would be easier to break down the remaining air in the direction $M B$, or otherwise the air in the direction $m y$, supposing the ship's mast to facilitate the progress in that direction.

18. Let the charge however strike in the direction $m y$, and so fall upon the mast,—then in proceeding to its ultimate destination, viz. the plane of the sea B , its course is still determined by the same general principles; that is to say, it seizes upon all those bodies which tend to assist its progress, and which at the same time happen to be placed in certain relative positions, *and upon no others*, falling with destructive effect upon intervening bad conductors, and exhibiting in non-conducting intervals all the effects of a powerful expansive force. If we examine carefully the course of discharges of lightning on ships in some hundred instances in which damage has ensued, we shall find this effect invariable. The damage has always occurred where good conductors cease to be continued, and the destructive consequences most apparent are those usually produced by expansion. The calorific effects, except as depending on this cause, are really inconsiderable; there are comparatively few instances in which metallic bodies have been fused, and no instance in which a bolt or chain of any considerable magnitude has been even much heated.

The following experimental and natural illustrations of these facts will be found conclusive and interesting.

Exp. 1. Lay some small detached pieces of leaf-gold a, b, c, d , &c. on a piece of paper, as represented in fig. 4; pass a dense shock of electricity over these, from the commencement at A to the termination at B , so as to destroy the gold; the line which the discharge has taken will be thus shown by the blackened parts; the result will be as in fig. 5, in which we perceive the course of the discharge has been in the dotted line a, b, d, e, f, g, h, i , being the least resisting line; and it is particularly worthy of remark, that not only are the pieces c, k , untouched, being from their positions of no use in facilitating the progress of the charge, but even portions of other pieces, which have so operated, are left perfect, as in the transverse piece i and portions of a, b, d, e , and f ; so little is there any tendency to a lateral discharge, even up to the

point of dispersion of the metallic circuit in which the charge has proceeded; indeed, so completely is the effect confined to the line of least resistance, that percussion powder may be placed with impunity in the interval between the portions *c*, *d*. Now the separate pieces of leaf-gold thus placed, may be taken to represent detached conducting masses fortuitously placed along the mast and hull of a ship.

Exp. 2. Let a thin continuous line, *m*, *n*, be passed through the separated pieces, and a dense accumulation discharged over the whole, as in the preceeding case. The effect will be as represented in fig. 6.; the discharge will be confined to the line of least resistance; and we may perceive in this, as in the former case, that those pieces, or parts of pieces, out of the track of the discharge, are not affected; thus a part only of the piece *g* is destroyed, also of the piece *i*, whilst other pieces, *b*, *d*, *e*, *f*, *l*, which in the former case, where the continuous line, *a*, *b*, was not present, were blackened by the discharge, remain here perfect.

Exp. 3. If the continuous line A, B (figs. 7, 8) be assisted by other comparatively short collateral branches, as *d e*, *d c*, having one common connexion at B, then a discharge which would destroy the line A, B, will divide upon these auxiliary lines, and the part *d*, B will either escape, or the whole will suffer together.

Exp. 4. Pass a discharge over a strip of gold-leaf, as A, fig. 2; every part of it, as indicated by the last experiment, will participate in the shock; and if it be of uniform density and thickness it will be everywhere equally affected, so that one portion will not be destroyed without the whole. This result will be readily distinguished from that represented at *d* and *i*, fig. 5, where the masses lie across the track of the discharge.

The diagrams here referred to, are copied from the actual effects of the electrical discharge in the way above mentioned.

19. These experiments are instructive. They evidently prove, that an electrical explosion will not leave a good conductor, constituting an efficient line of action, to fall upon bodies out of that line. Mr. Sturgeon's assertion that a conductor on a ship's mast would operate on the magazine is therefore quite unwarranted. Besides, we have many instances of the masts having been shivered by lightning into the step, whilst acting as partial conductors, without any such consequence; as happened in the *Mignonne* in the West Indies, the *Thetis* at Rio, the *London*, *Gibraltar*, *Goliath*, and many others. Instead, therefore, of a conductor on the mast being dangerous, it is absolutely requisite as a source of safety to the

ship, by confining the discharge to a given line and leading it to the sea.

20. It was from a careful consideration of the common effects of lightning, and from such experimental facts as those above mentioned, that I was led to suggest the propriety of fitting continuous conductors of lightning of great capacity in the masts of ships, linking them by efficient communications, together with the principal detached metallic bodies in the hull, into one general continuous system, and finally connecting the whole with the sea. These conductors consist of two laminæ of copper-sheet, varying from one inch and a half to five inches wide, and being together nearly one-fourth of an inch thick; they are inlaid so as to be fair with the surface of the mast, and form a series of shut-joints; they are otherwise so constructed as to present an uninterrupted line of action from the highest point to the sea. The method has been partially used in the British navy for several years, and has been proved in every way efficient. In no case has any of the vessels fitted with them received the slightest damage, although frequently exposed to severe thunderstorms, and in some instances actually struck by heavy discharges similar to that which fell on the *Rodney* in December, 1838.*

21. If we consider attentively the effects of this shock, we shall find them in complete accordance with the principles just stated. The attendant phenomena were of the simplest kind, and such as have always occurred in cases of ships struck by lightning not having a continuous conductor: e. g. the electrical discharge, in forcing its way between the sea and clouds, over resisting intervals, and between discontinuous metallic masses, was productive of a violent expansive effect in these intervals; causing at the same time a considerable evolution of heat. There was really nothing particularly remarkable in this instance; the course of the discharge was a very simple affair, being, according to the law of electrical action just exemplified (Exp. 2,) in the line or lines of least resistance from the highest point to the sea: thus the course of the discharge was, as represented in fig. 6, plate XI, along the masts and rigging, upon the general mass of the hull and sea. The vane-spindle *a*, upon which the accumulation was first concentrated, was of course severely dealt with. From this, being probably assisted by the moisture on the surface of the wood, it glanced over the royal pole to the head of the top-

* See a letter in the *Annals* for January last, by Lieutenant Sullivan, R. N., who witnessed these effects.

gallant mast at *b*, where it found intermediate metallic assistance in the copper funnel for the top-gallant rigging: from this, the resistance in the mass of the wood appears to have been less than that on its surface, probably from the long interval of air between the funnel and conducting bodies about the cap below, the mast was therefore split open as far as the cap at *c*. Here again it was enabled to strike over the surface of the mast, upon the metals about the parrel of the top-sail-yard at *d*, where the accumulation became again concentrated, producing a powerful expansion and heating effect so far as the lower cap at *e*; and thus it passed along *per saltum* over the lower mast *m*. from one metallic mass to another, until within a striking distance *s* of the sea and hull, it divided upon the hull and sea in convenient directions *s n*, *s o*, *s p*. In this course, as indicated by the waving black line *a*, *b*, *c*, *d*, &c., it evidently sought assistance from all the conducting matter it could seize upon; such as the wet ropes, the copper funnel for top-gallant rigging at *b*, the iron work and other bodies about the topmast cap at *c*, as also the men in the top-gallant cross-trees at *c*. The charge evidently divided upon them in proportion to the assistance each could afford as a small auxiliary circuit, as Exp. 3; the men nearest the mast would be necessarily in the more direct course of the discharge, the others would be more or less so according to their respective positions; that these poor fellows who were killed suffered in this way as being conductors to parts of the charge is evident from the appearance of the bodies. Mr. Sturgeon calls especial attention to the circumstance of the men being thrown in opposite directions, and thinks it remarkable: but how could it be otherwise? the intervening air being caused to expand violently from a central point, would necessarily operate as a central force; surely there is nothing very new in this.* About the parrel of the topsail-yard at *d*, we should expect again power effects; for here again the charge became concentrated, and set the sail, &c., on fire. This is quite in accordance with the known laws of electrical action; thus we find the points of ingress and egress of an artificial charge, when caused to fall on a slip of gold-leaf or other matter, are always those in which the most powerful effect arises; and when we desire to fire inflammable matter by electricity we place it directly between detached metallic points.

22. The circumstance of the lightning striking over portions of the wet mast without damage, is precisely the same effect as observed in certain cases of artificial electrical discharges.

* Certainly nothing new, merely an instance of the effects of lateral explosion of the first kind. EDIT.

Thus a very slight film of moisture will allow a jar intensely charged to discharge a luminous ball over a long strip of glass. Dr. Franklin found he could destroy a dry rat by an electrical shock when he failed to hurt a wet one. If we continue to follow the discharge we find similar expansive and destructive effects; such as the bursting of the hoops on the mast, &c., &c., which will sometimes occur and sometimes not.

23. There is really nothing in all this to call for especial remark, except we may observe, as shown by the experiments already described, that if a good capacious conductor had been incorporated with the mast from the truck to the metallic masses in the hull and to the sea, then these *expansive* and destructive effects could not possibly have occurred; since the interrupted circuit would have been avoided, and the intense electrical action have vanished, or nearly so, at the mast-head, for it would have no longer been driven to force its way in a dense explosive form to the hull and sea; of this we have the most complete evidence from experience, particularly in the cases of the ships struck by lightning having such conductors as those just alluded to, curiously enough quoted by Mr. Sturgeon as evidence to the contrary. It seems a strange way of disproving a fact to quote those who, having been eye-witnesses, insist upon its truth. That the electric matter finally distributed itself upon the hull as *well* as on the sea, is evident from the circumstance of the casing of Hearle's pump at *t*, which led through the side under water being shivered; from the vivid electrical sparks below, and from the usual smell of sulphur in the well, and appearance of smoke in the orlop-deck.

24. The interrupted circuit therefore to be traced here, is first from the vane-spindle to the copper funnel of top-gallant rigging; 2nd, from this to the conducting bodies at the heel of the top-gallant mast; 3rd, thence to the metallic masses about the parrel of topsail-yard; 4th, between this and the metallic bodies about the head of lower mast; 5th, from this over the detached metallic bodies on lower mast; finally, from lower mast to the hull and sea. The effect of this shock of lightning appears to have been somewhat palliated by heavy rain.

Although Mr. Sturgeon has gone far out of his way to twist these phenomena into an accordance with certain theoretical views, and sets them up as being of an extraordinary kind, they are nevertheless of a very simple character, and are merely illustrative of a few well-known laws of electrical action.

(To be Continued.)

LXXII. *Mr. Sturgeon's fourth Letter to W. Snow Harris, Esq. on the subject of Marine Lightning Conductors.*

SIR,

I had expected that the fury of your wrath against the exposé, contained in my fourth memoir, of the probable danger and unnecessary expense consequent upon your plan of lightning conductors being established in the royal navy, had been totally vented in your first unprecedented volley of abuse; but in this expectation, as well as in that of your being a scientific reasoner, I have been sadly disappointed; for instead of keeping "close quarters," and observing that strict candour which ought to be held sacred in scientific discussion, and especially on a topic of such high national importance as that of marine lightning conductors, you still keep raving on, as if determined, by your coarse bullying language, to crush every attempt to scrutinize your plan of conductors, or any notice that may be taken of those errors into which you have obviously fallen. Such asperous domineering may probably be suitable enough in your hands, as it is the principal weapon you employ. But as I am not in possession of any of the kind, nor of any desire to *shine* in a contest of such an ignoble character, I most willingly acknowledge you as master of that part of the field.

That point settled, I must now solicit your attention to a few particulars of a somewhat more important character, and first of all to your own confession of your own ignorance of certain points of electric action. *Occasionally* you deny the existence of *lateral* discharges in toto: and, *occasionally*, you admit some kinds of lateral discharges and deny others. Perhaps you will acknowledge that the mere denial of a fact is no proof of its non-existence; and that it may as possibly be grounded on the mere ignorance of the party denying it. Moreover, your denying a fact at one time and acknowledging it at another, is no sure indication of your accuracy, in either case, emanating from a sound judgment.

You have admitted, however, in page 317 of this volume that there is such a phenomenon as a *lateral explosion*, and you have admitted also, that this lateral explosion produces mechanical action, hence, you have, *indirectly*, admitted all that I have advanced concerning lateral discharges of the *first kind*.* For, although you seem to have no idea at all of

* See paragraph 193 of my fourth Memoir, page 174 of this volume.

an *electrical wave*, you ought to have known that such a wave must necessarily be produced by the expansive force of the explosion; and had you been that *practical* man, that I have all along expected you were, you would have known that a gold leaf electroscope properly exposed would indicate an electrical *wave* during the occurrence of a flash of lightning; and that a similar wave is produced by artificial discharges. Such facts, however, appearing to be quite unknown to you I shall not, here, trouble you more about them.

I cannot but admire your mode of attack in the fourth paragraph of your second production on this troublesome memoir of mine. You seem to be labouring under some uneasiness about my few "*clumsy experiments*," which notwithstanding your self-sufficient strong position in the scientific world, seem, by paragraph 2, to have produced an apprehension that they may possibly open the eyes of those whom *your elegant experiments* before the Navy Board, and *highly scientific illustrations* of the effects of lightning on ships' masts before the British Association, the members of the United Service museum, and other bodies, have so long been blinding.

Now, it so happens, that those few "*clumsy experiments*" of mine, "*with some every-day phenomena of the electrical kite*," are the very facts which an inventor of a lightning rod ought to be perfectly acquainted with; and I verily believe that, had *you* been sufficiently familiar with the "*every-day phenomena*," as you are pleased to call them, you could never have been led to the persuasion that the effects experienced on board the *Beagle* and the *Dryad* were any indications of those ships being struck by lightning. Every sailor knows well that ships are severely shaken by a peal of vicinal thunder, though no lightning strikes the vessel; and if you would condescend to repeat some of my "*clumsy experiments*" with the electrical kite, during a lightning storm, you would soon learn that the "*hissing noise*" and other phenomena, witnessed on board the *Beagle* and the *Dryad*; are the constant productions of electric waves in the atmosphere; sometimes from flashes of lightning and sometimes from the mere transit of a cloud over the kite. Moreover, a true indication of the *Beagle* being *not struck* by the primitive discharge is, that neither her compasses nor her chronometers were magnetized by the event; for had the main-masts' conductor transmitted a flash of lightning as has been supposed, no circumstance hitherto brought to notice, could possibly have prevented the magnetization of the steel in the chronometer which was placed so near to that conductor.

Again, whatever may be your opinion of those "every-day phenomena of the electric kite," I have always considered that some of those which I have recorded are very far from being deserving of that epithet you have given them. They are obviously such as you never saw, and, I believe, they are such phenomena as you are unable to shew recorded by any other person. My electric kite experiments have probably been more extensive than those of any other person of the present day; and, to me, they have been more productive of correct views of electric action generally, than any series of experiments I ever before pursued; and led me to other investigations which otherwise I might not have thought of.

By the copious discharges occasionally exhibited at my kite-string, I gained a knowledge of atmospheric electrical waves, and of the causes of their production.* By an attention to the motions of the balls of a Cavallo's electro-scope, I have gained a knowledge of the variableness of the density of atmospheric electricity in windy weather. By prosecuting my kite experiments at all seasons of the year, for about six successive years, I have been enabled to foretel at what season of the year, and under what circumstances of weather, the atmosphere would be most powerfully electric with respect to the ground.

By making my experiments in places remote from each other, upon lofty mountains and in low valleys, I have been enabled to understand that an unclouded atmosphere is *constantly electro-positive* with reference to the earth. By studying this fact in connexion with electric waves, I have been led to a knowledge of the cause of the ground being *sometimes* electro-positive with respect to the air immediately above it. And by these and other fluctuations of the atmospheric electrical pressure, arising from hygrometrical, and thermometrical changes, &c., I have been enabled to understand the cause of the ever-varying electrical condition of bodies composing the surface of the earth.

By employing several kites at the same time, at different altitudes, at different seasons of the year, I gained a knowledge of the different electric conditions of atmospheric strata at those altitudes: and from a knowledge of the atmosphere being differently electric at different altitudes, I was led to infer that an exceedingly thin stratum of air would be differently electric on its upper and lower surface.—Keeping this

* See my fourth Memoir, page 181 of this volume.

idea in view whilst repeating some of the beautiful experiments described in Sir Humphrey Davy's Bakersian Lecture for 1826, I was led to suspect that thin strata, or films, of metallic bodies might possibly exhibit different electric action on their opposite surfaces, which I found to be the case, and in the year 1827, I constructed my dry electric column, having one metal only; each piece having a *relatively* positive and negative surface.*

From my success with the dry electric column, I was led to a "few clumsy experiments" in galvanism; by means of which I shewed that *metallic contact* is not essentially necessary to the production of galvanic action.† This discovery was thought of sufficient importance by Dr. Faraday, to deserve a place in the Transactions of the Royal Society, and to select it from my book as a fit subject for the theme of his 8th Series; forgetting, however, to associate the name of the discoverer with the fact. It was by contemplating the electrical character of the same kind of metal under different states of polish, texture, &c., that I was led to the discovery of making active galvanic combinations with one kind of metal only, and of shewing that *cast* and *rolled* zinc are in different electric conditions. It was in consequence of this discovery that I was enabled to shew that rolled, or hammered zinc in combination with copper, made more powerful galvanic batteries, than *cast* zinc, not so treated, would make in combination with that metal.‡ This discovery was also honoured with a place in the Philosophical Transactions of the Royal Society through the courtesy of Dr. Faraday, who, considering it sufficiently important to form a prominent feature of his own dexterity in the tactics of transplantation, very politely handed it to the Council of the Royal Society as a discovery of his own, placing it very conspicuously in his 10th Series.

The "every day phenomena at the electrical kite," which gave me the first idea of thin strata being differently electrical on their opposite sides, led me to the supposition that the thinnest films which constitute metallic crystalline groups might also be in different electrical conditions. This idea led me to an extensive series of "clumsy experiment" which were perfectly successful in shewing that each separate metal is susceptible of exhibiting thermo-electric currents, and that

* See my Experimental Researches in Electro-Magnetism, Galvanism, &c. p. 64. Published in 1830, by Sherwood, Gilbert and Piper.

† *ibid* page 21, 83, and 84.

‡ See my Experimental Researches, &c. page 65—74.

each group of the crystalline films, is, in fact, an electrical pile, as decidedly as any electric pile formed of two or more distinct kinds of metal.*

These few specimens of the consequences of *some* of my "clumsy experiments" and "every-day phenomena at the electric kite," may probably afford you an idea of their having been viewed, by Dr. Faraday and others, under a very different aspect to that which would fain place them in. And, indeed, from the tenour of your first letter to me,† I have every reason to think that, your present disingenuity and want of candour are the mere effects of the lamentable *Electrophobia* under the torments of which my fourth memoir has so unhappily placed you: and that, as the ardour of the fever abates, I am in hopes your mind will gradually be restored to its usual healthy tone of vigour and conscientiousness; and resume its capability of appreciating the labours of those who, even under inexpressible disadvantages, have so long been working with you in the same field of science. Hence it is that, notwithstanding the violations of courtesy and candour which you have manifested during the impulse of those fervent paroxysms under which you have been unhappily labouring, I most willingly and sincerely exonerate you from all blame in this temporary misunderstanding; and you may rest perfectly assured, that, whenever liberality and candour again emanate from your pen, they will be accompanied by my best wishes for your welfare and success.

I have the honour to be

Sir,

Your obedient Servant,

WILLIAM STURGEON.

To W. Snow Harris, Esq.

P.S.—The state of the controversy will be seen on the next page.

* Philosophical Magazine.

† See page 325 of this volume.

Balance Sheet ; or the present state of the Controversy.

MR. STURGEON,	MR. HARRIS.
Has shewn that the experiments made before the Navy Board, at Plymouth, were <i>inconclusive</i> ; and that the results were not due to any superiority of Mr. Harris's lightning conductors.—Page 163.	Has <i>not denied</i> the inconclusiveness of his experiments made before the Navy Board, at Plymouth, nor attempted to shew any other experiments that are more favourable to his system of conductors than to other systems.
Has shewn that there are three distinct kinds of <i>lateral discharge</i> , and has described the <i>first kind</i> and its mechanical effects.—P. 174.	Has <i>indirectly acknowledged</i> the <i>first kind of lateral discharge</i> , and also its mechanical action.—P. 317.
Has described the <i>second kind of lateral discharge</i> , at page 174. He now refers the reader to the <i>Athenæum</i> for Sept. 30th, 1837. Page 717, Mr. Addams stated that "he had once seen upon the discharge of a large electrical battery, a wire splendidly illuminated by the lateral discharge, and exhibiting the corrugations spoken of by Professor Henry."	Denies this kind of lateral discharge in the present discussion, page 318 of this vol. but, told the British Association, at Liverpool, that "he had produced beautiful illuminating effects by discharging electricity along a wire enclosed in an exhausted glass receiver."— <i>Athenæum</i> for Sept. 30th, 1837.
Has described the <i>third kind of lateral discharge</i> , and shewn the cause of its production.—P. 174.	Acknowledges this kind of lateral discharge; but gives a different explanation of the cause.
Has contemplated the electrical condition of a lightning-rod, during the time of its carrying a discharge; and the effects consequent upon that electric condition.—P. 174, and 418, 419, of this vol.	Has contemplated the electrical condition of a lightning rod, <i>prior</i> to the discharge taking place, and consequently when no lightning was present, nor any conductor necessary.—P. 316, 317, 419, of this vol.
Has described, and shewn the effects of, <i>electric waves</i> .—P. 174, 180, 181, 182, of this vol.	Has not taken into consideration <i>electric waves</i> .
Has described the electro-magnetic phenomena consequent upon a flash of lightning traversing the main-mast conductor of H. M. S. Beagle; and has shewn that, as no magnetic effects were produced, the probability is, that the primitive flash of lightning <i>did not strike</i> the vessel; the observed effects being due to an electric wave.—P. 179.	Insists on the Beagle being struck; but for want of due attention to the influence of electric waves; and the electro-magnetic influence of a primitive discharge of lightning, he has not taken their effects into consideration.
Has taken into consideration the <i>probable effects of oblique flashes</i> of lightning on the rigging of ships.	Has not noticed the probable effects of oblique flashes of lightning on the rigging.
Has disposed his system of conductors so as to prevent, as far as possible, the effects of oblique flashes, in the rigging.	Has disposed his system of conductors so as to afford <i>no protection</i> whatever to the exterior rigging, against oblique flashes.
Has disposed his system of conductors so as to distribute lightning into several branch conductors, and scatter its effects into comparatively harmless streams, almost the moment it arrives at the rigging.	Has disposed his system of conductors so as to give <i>no assistance</i> to each other above deck.
Has disposed his conductors so as to prevent the lightning entering the ship, by carrying it overboard on both sides.	Has disposed his system of conductors so as to lead the lightning into the body of the vessel.
Has disposed his system of branch conductors, so as to counteract each other's magnetic effects on steering compasses, chronometers, &c. placed near to the axis of the vessel; whether those instruments be placed above or below deck.	Has disposed his system of conductors so as to give great facility to magnetic action on compasses, chronometers, &c. placed either on deck or below.

The difference of the expense, time requisite for the equipment of a 50 gun frigate with the two systems of conductors, may be seen at page 190 of this volume.

LXXIII.—*Voltaic reaction, or the phenomenon usually termed Polarization.* By Mr. W. R. GROVE.

On Friday evening, the 13th instant, Mr. W. R. Grove, delivered in the Theatre of the Royal Institution, of Great Britain a very interesting discourse on "Voltaic reaction, or the phenomenon, usually termed Polarization," and at the conclusion, exhibited the effects produced by his new and surprising voltaic combination.

We feel it impossible to condense the subject as treated by Mr. Grove, it should be given verbatim through each step of his inductions, and well deserves full publication; we are happy however to be able to furnish some particulars as to the astonishing performances of the batteries employed, which the audience were informed were prepared for the occasion by Messrs. Watkins and Hill, of Charing Cross.

Mr. Grove had his batteries arranged in two forms, one developed the condition of "intensity," while the other exhibited "quantity," and from the effects observed, it was manifest that in both cases a most judicious arrangement had been adopted.—The "quantity" battery consisted of 40 pairs of 4 inch square platinum plates, with double amalgamated zinc plates with porous cells and porcelain troughs arranged in series of eight, 5 pairs of plates.—Nitric acid being in contact with the platinum, and a diluted acid solution of sulphuric acid and water in contact with the amalgamated zinc.—The platinum and amalgamated zinc plates were arranged in their proper cells, and supplied with acid sometime previous to the lecture, and it was noticed that no action was visible, indeed all apparently was in perfect repose for three hours, until the moment of completing the circuit, by joining the positive and negative ends of the battery by a conducting substance. The wonderful energy of the arrangement then developed itself. In the first instance with the "quantity," arrangement of eight 5 pairs, each 4 inches square, when the circuit was completed through a large voltmeter prepared for the occasion, *one hundred and ten cubic inches* per minute of the gases was evolved by the decomposition of water.

We believe this is the first instance wherein so large a quantity of gas has been produced by voltaic action in the short period of a minute.

The heating effects of this quantity battery were far above that obtained by any previous voltaic combination, for it fused a thick iron wire $\frac{1}{8}$ th inch diameter, and 10 inches

long, and a slip of thick platinum foil 12 inch long and 1 inch in breadth, was rendered white hot.

Mr. Grove then referred to his "intensity" battery which only covered a square surface of 16 inches on each side. The cells were 4 inches high and consisted of 50 pairs of platinum plates 2 inch \times 3 inch, with double amalgamated zincs. With this comparatively small intensity-battery an arc of flame, between charcoal points, was observed $1\frac{1}{4}$ inch long fused large and thick iron wire. Mr. Faraday having lent his pocket knife to the lecturer, the large blade was instantly deflagrated, exhibiting a splendid shower of scintillations of steel. Large masses of zinc, copper, soft iron, &c. were then submitted to the action of the battery, and a most splendid series of combustions were the consequence, the colour of the light being dependent upon the metal employed.

Mr. Grove adverted to the letter of Professor Jacobi, of St. Petersburg, to Mr. Faraday, published five months after his, Mr. Grove's first paper on the battery, in which it was stated some extraordinary effects of decomposition and magnetic action had been obtained by a battery, of which the Professor did not give the particulars.

Mr. Jacobi however has since this, without hesitation, acknowledged to Mr. Patterson, who witnessed his experiments last October, that the voltaic combination, he employed, was that of Mr. Grove as originally published in the *Compte Rendu* of the French Academy.

We cannot conclude this hasty sketch of the evening's exhibition, without expressing our conviction that Mr. Grove has rendered a great boon to science, by the publication of his researches on voltaic combinations, and we trust he will continue his labours in this fertile department of experimental science.

LXXIV.—*Review and Notices of New Books.*—*An Outline of the Sciences of Heat and Electricity.* By THOMAS THOMSON, M. D. *Regius Professor of Chemistry in the University of Glasgow, &c. &c. &c.* Fl. Baillière, 219, Regent-street, London. J. B. Baillière, 13, Rue de L' Ecole de Medicine, Paris: T. O. Weigel, Leipsig.

The work before us contains the best treatise on heat that we have yet seen, and is another specimen of that eminence of talent and exquisite discrimination by which its author has,

for a long series of years, distinguished hyhimself in the fields of science. The phenomena of heat, at all times, display a fine field for the contemplation of the philosopher; and when arranged in that judicious manner, treated in that easy and lucid style, and discussed with that freedom and candour in which they appear in this work, they become familiar and interesting in every department of experimental science, and easily applicable to many of the common affairs of domestic life.

The work is printed on good paper, and with a bold clear type; and contains many illustrative wood engravings and a copper plate. It is got up in a neat style, and ought to find a place in every scientific library, both public and private: and we strongly recommend it to the perusal of families who wish to obtain information in these subjects on which it treats.

LXXV.—MISCELLANEOUS.—*Description of an Electrical Machine, made by Messrs. Watkins and Hill, 5, Charing Cross, London, expressly for the Royal Victoria Gallery for the Encouragement of Practical Science, Manchester.*

Figure 5, plate XI, is a perspective view of this splendid instrument, *f, f, f, f*, is a stout rectangular mahogany frame, supported by four pillars at its angles, as seen in the figure. From each of the two long parallel sides of this frame, rise two mahogany pillars, supporting a cross piece, which form a *vertical* rectangular frame on each side of the horizontal one. The front vertical frame is represented at *e, e, e, e*; and the rear one, which is principally hid from view, shows one of its pillars at *e*, on the right hand side of the picture; its other pillar and cross piece are represented by dotted lines. From each end of the horizontal frame *f, f, f, f*, rises a stout glass pillar *g' g'* and *g' g'*: surmounted with a brass ball *b* and *b*. These glass pillars, and another shorter one at *P*, support the curved brass conductor *o, o, o*: and also four rubbers, two on each pillar, with their silken flaps *s s*, and *s, s*. The glass plate, represented by the large oval, is four feet in diameter: and revolves remarkably true, on a stout brass axle, supported in the middle of the cross pieces of the vertical frames *e, e, e, e*, and *e*, &c. The vertical brass rod *r*, screws into the ball *b*, and is intended to keep the upper silken flap from being displaced, by connecting them with silken cord in the manner shewn by

the zig-zag lines. The lower silken flap is kept in its place by attaching it, in a similar manner, to its vicinal glass pillar *g g*. The prime conductor *c, c, c, c*, is a splendid appendage to the machine. It is of brass, beautifully polished and lacquered, and furnished with a stout solid glass stem *g, g*, which, by means of a brass dovetail is attached to the cross piece of the front vertical rectangular frame *e, e, e, e*. The collecting points proceed from two straight brass tubes terminated with ebony balls as seen in the figure. The massiveness of the frame work, the unusual size and elegance of the prime conductor, and the excellency of workmanship displayed in the whole, give to this machine a degree of magnificence, perhaps never before equalled in any piece of electrical apparatus. Its power corresponds with its magnitude and appearance, and, if possible, surpasses my anticipations at the time I gave directions for its structure. I have already passed through a series of twelve Lectures on electricity since the arrival of the machine at this Institution, and its performance has given both our Directors and myself the greatest satisfaction.

WILLIAM STURGEON.

Royal Victoria Gallery, for the Encouragement of
Practical Science, Manchester.

METEOR SEEN AT SANDWICH, KENT.

Letter to the Editor on a Singular Meteor seen at Sandwich, Kent.

SIR,

About midnight on the 6th inst. a most extraordinary Meteor was seen by many persons in these parts, but I had not the good fortune to be one of the observers. Persons with whom I have conversed say, it was as large as the moon at full, the light intensely brilliant, even more so than the brightest day light, and that our gas lamps for a few seconds were reduced to insignificant specks of light. It passed to the N. W. I hope you will get some better account of this phenomenon.

W. H. WEEKES.

Sandwich, Feb. 18th, 1840

Note on Voltaic Batteries.

In my "*Experimental Researches in Galvanism, &c.*" published in 1830, I have stated that, if any method could be devised for keeping the transfer of the mercury to the copper, amalgamized zinc might be very advantageously employed in the structure of voltaic batteries. During the course of experiments which I was then pursuing, I discovered that, although the electrical powers of copper, and some other metals, became deteriorated by a partial coating of mercury, iron, on the contrary, had its electrical powers improved by such a coating of mercury, and formed with amalgamated zinc, a more powerful battery than copper with zinc.

About a year ago, I formed a battery of twelve iron gas tubes, each twelve inches high ; and strips of amalgamated zinc, which performed remarkably well. I am now making a very extensive battery of similar materials for this Institution, which I shall describe in the next number of the *Annals*.

About a month ago, Mr. J. P. Joule purchased one of Grove's batteries of Watkins and Hill, and has been led to try sheet iron instead of the platinum. That gentleman informs me that the iron performs very well.

WILLIAM STURGEON.

*Royal Victoria Gallery of Practical Science,
Manchester, March 10th, 1840.*

END OF VOL. IV.

THE ANNALS
OF
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AND
Guardian of Experimental Science.

CONDUCTED BY

WILLIAM STURGEON,

Lecturer on Experimental Philosophy, at the Honourable East India
Company's Military Seminary, Addiscombe, &c. &c.

AND

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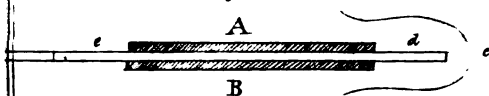
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Fig 1.



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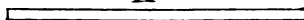


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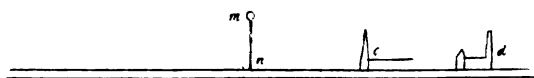


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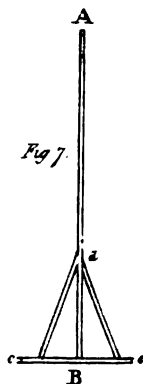


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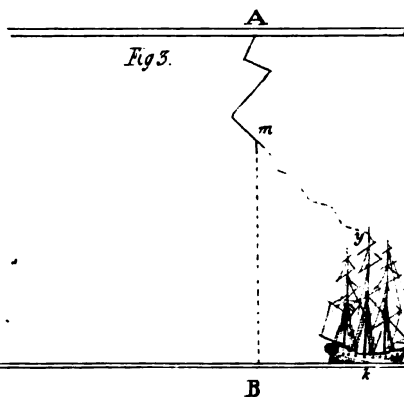
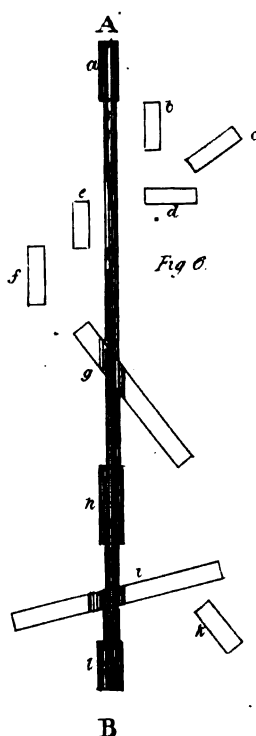
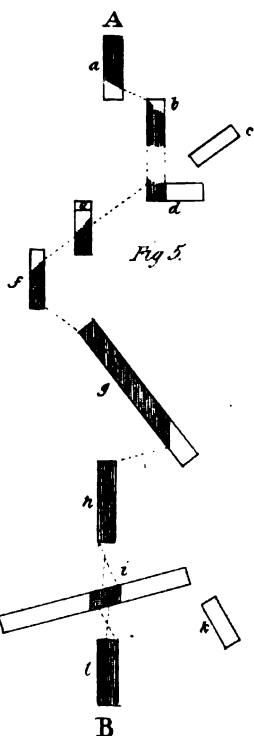
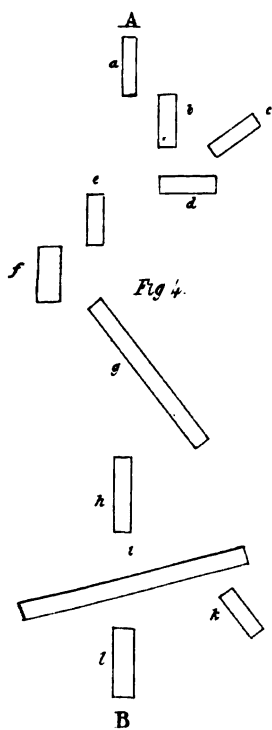
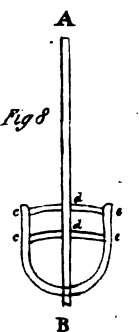


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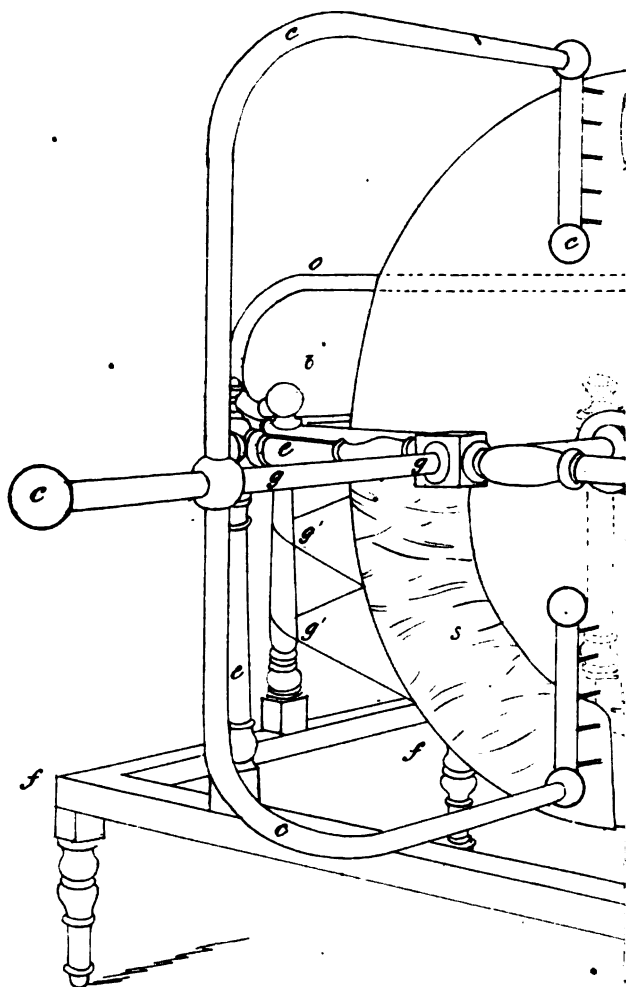
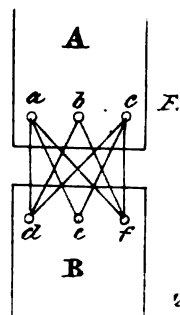
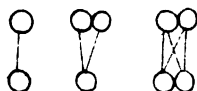


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